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A THEORY OF THE CAUSATION,

AND

SUGGESTIONS FOR THE PREVENTION,

OF

DYSENTERY:

TOGETHER WITH

HYPOTHESES ON THE CAUSATION,

AND

VIEWS AS TO THE PREVENTION,

OF

TYPHOID, CHOLERA, YELLOW FEVER, REMITTENT,
DIPHTHERIA, TYPHUS, AND OTHER ZYMOTIC
DISEASES IN MAN AND ANIMALS.

BY

MUCOR.

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PREFACE.

I HAVE very little to say to the reader—lay or professional. There is nothing new under the sun. Possibly the views herein submitted come within the spirit and meaning of the saying. Yet it will be found that, if they are not new, they have at least been some time in abeyance. Or it may be they are not true. They are startling enough to look unsound; and their consequences, if sound, are so stupendous, that I can hardly bring myself to believe there is not some fatal error, somewhere, in the induction. At the present moment, a successful attempt to unravel the causation, and to indicate means for the prevention, of so many of the most deadly plagues of the world, seems highly improbable, even if it be not regarded as almost an impossibility. I am fully conscious of the distrust with which propositions, having an air of such extreme incredibility, may be at first received. I can conceive they will meet with a hearty opposition. I am prepared to see them rather roughly handled. Yet as I am myself unable to detect any flaw in the reasoning, I send them forth for others to examine. They may, at all events indirectly, if not directly, serve some useful purpose.

Whilst, however, the greater number of these views are, admittedly, purely speculative, and whilst conclusions based upon assumed data may be looked upon with just suspicion; I must lay claim to one substantial discovery—even though the absolute proof be now wanting. I submit that the specific poison of Dysentery has been clearly traced to its source. And, further, that an easy mode of preventing that disease, in any, and in every, country, has been pointed out. If the facts collected, and the inferences drawn from them, do not sustain this, I concede at once that all the propositions as to the causation of other diseases are worthless. If the reader, after examination of the argument touching Dysentery, shall consider it insufficient to establish the case put, he will of course discard the remainder. If the reasoning breaks down here, I shall yield every other point. In fine I am content to leave it to this issue. For, indeed, if I am wrong in the induction as regards Dysentery, I cannot be right as to the causation and prevention of Typhoid Fever, Diphtheria, Cholera, &c.

But it was on the strength of the conviction that I had solved the problem of the long-hidden cause of Dysentery, that I made the attempt to ascertain the cause of Typhus, Typhoid, and Remittent, Fevers, Diphtheria, and Cholera. What measure of success I have had, is for the learned to say, or for the future to determine. I am sanguine enough to believe, that, if the doors to the sources of all these maladies have not been actually thrown open, the master-key by which they will eventually be unlocked, is to be found in these pages. It will be understood, however, that I consider the conclusions as to the origin of some of these diseases, to have a far less solid foundation than the conclusions as to the origin of Dysentery. The probabilities that the problem of Dysentery has been correctly solved, are many greater than the probabilities that the true cause of Typhus has been made out. Calculating these probabilities, and fixing those of Dysentery at 100, I take it they would stand somewhat in this relation.

Dysentery	100
Typhoid	80
Cholera	75
Yellow Fever	72
Remittent	50
Diphtheria	20
Typhus	5

Thus it will be seen I am least confident about the cause of Typhus; though the hypothesis of the causation of Diphtheria took me far longer to shape. Indeed it was only at the last moment that I succeeded in getting hold of the idea. This will explain the fact of the views being at the end of the work. I regret extremely that I had not more time to work out the details, and to put the hypothesis into a more presentable form. Many other points could have been made—such, for instance, as the striking advantage of a removal from the house; of the well-known cases where the source of infection has been so closely connected with a dwelling, that successive families living in it have been attacked one after the other; and the fact that the Chinese in this colony have hitherto had an immunity from the disease—as was shown by the evidence taken by the Royal Commission. These and many other reasons, which escaped me at the moment of writing, all strongly support the hypothesis submitted. They will, however, as readily suggest themselves to others. As the matter stands, the view as to the causation of Diphtheria, seems more likely to be correct, than that as to Typhus, in the proportion of four to one. If it should, happily, turn out to be right, the prevention of Diphtheria will be as simple as that of Dysentery.

My first intention was only to have submitted the thoughts that had, I may say accidentally, occurred to me on the subject of Dysentery. As I wrote on, I found that the causation of Typhoid and Typhus were so mixed up with that of the Flux, that I could not well omit all reference to these camp diseases. I soon began to perceive, also, that Cholera was closely allied. Yellow Fever was clearly of the same family. Remittent too, and Diphtheria, were evidently of the class. So that as I warmed to the work, I had to enlarge its scope and design. This will account for the want of arrangement and for the severance of things that should have been kept together. It has given a patchwork appearance and rather a slovenly look to the book; but this is a trivial matter, and I have no intention of deprecating criticism on account of it. With the exception, perhaps, of those portions relating to Dysentery, the whole work is but a bundle of hypotheses, rapidly thrown together after being anxiously thought out. I have been too intent on the matter to think of the manner. It is now alluded to in order to explain the *progressive* nature of the views. As the proofs have come from the printers, I have been made aware of some slight discrepancies here and there, and some little incongruities even; but as they have been on minor points, I have let them stand. They are merely parts of the scaffolding of an unfinished building, run up in haste. For it is now barely six months since the cause of Dysentery first occurred to me; and those who can appreciate the mental labour involved in some of the propositions—

whether sound or unsound—will readily see that I have not had much time for elaboration. This will be more especially evident when it is mentioned that I have had other work to do. The spare hours of a man's time during six months. have proved all too few for more than saying what I had to say in the plainest possible language, and in the most direct way. For I have been deeply impressed with the importance of the subject; and I have felt from the first that, if the conception of the leading idea was sound in principle, unnecessary delay in embodying it in words, would be almost criminal.

The opinion I have formed upon the subject of Malaria, of course depends upon the soundness of the views as to the causation of disease. If they are correct, it follows that there is no such thing as indigenous Malaria, with one exception, in any part of the globe; that every form of Malaria, but one, is an artificial product of man's own creating; and that, therefore, every country under the sun may be converted, by the agency of man, from the most malarious state, into the most wholesome condition. This is a large proposition, but I think it has been demonstrated herein. Only one nation in the world has practically shown how Malaria may be obviated. For hundreds of years, Japan has been entirely free from every plague and pestilence. Although their near neighbours, the Chinese, have suffered from all forms of eastern zymotics, the Japanese have not had a single epidemic. Neither Cholera, nor Dysentery, nor Typhoid, nor Remittent, nor even Dengue, has once laid hold of any of their towns, or cities. The principle on which this eminently civilised people have achieved this marvellous result, is fully explained, as I venture to believe; although, I regret to say, I am unable to furnish some of the details connected with the actual mode of carrying out their measures. If, however, it should happen that there are any Japanese in England, those hygienists who would not think it beneath them to learn from such a source, might possibly get some valuable hints on the subject of dealing with masses congregated in large cities, such as Yeddo.

It has been said that there are certain sets, or cliques, of scientific men, in London; and I have heard, that unless a writer shall happen to get his work properly accredited, no matter how valuable it may be, it will drop into oblivion, or will be written down. I take leave to doubt this. There may have been instances of a work being written up. But I do not see how views that have anything in them, are to be suppressed by any set of men. So I send these thoughts out to sink or swim on their merits. The questions raised are too large to be shelved altogether. They will have to be dealt with, somehow, sooner or later. They will be determined finally, one way or the other, by the microscopist.

As I shall naturally take a most lively interest in all that relates to the subjects of which I have treated, I shall be grateful for any information that may be furnished to me from any part of the world, either tending to confirm, or to upset, the views I have formed. A letter addressed to Mucor, Post Office, Melbourne, or to the Publisher, Melbourne, will reach me. This book, by the way, is published anonymously for many reasons. One is, that if I have succeeded in suggesting an efficient cause for, and a certain prevention of, some largely fatal diseases, by simple induction, the mental gratification and the quiet satisfaction at the good effected, will be my reward. If, however, the argument is faulty, I may, perhaps legitimately, spare myself the annoyance that a well-intentioned, but weak-minded, attempt to alleviate human misery, might entail.

MUCOR.

MELBOURNE, *March*, 1873.

INTRODUCTORY VIEWS ON THE CAUSATION OF DYSENTERY.

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1. **Dysentery** has been known in all ages, and in all countries —except, perhaps, in those bordering on the Poles. There was an epidemic, however, in the Krageroö District, in Norway, in 1859; thus showing that it is to be met with in tolerably high latitudes. It is, therefore, the most widely distributed, and has, perhaps, proved the most largely fatal of all diseases. Whether the true source of this pestilence has been demonstrated within the last few months, I have no certain means of knowing. But it is quite clear that up to a very recent period, the cause of dysentery had not been determined during the centuries it has ravaged mankind. One has only to recall to recollection the losses of the British, French, and Turkish troops before Sebastopol, to assure oneself that up to that time, at all events, the real origin of this scourge of armies had never been suspected, or provided against.

2. In order to exhibit more clearly the present position of our knowledge upon the subject of the causation of dysentery, and also to facilitate the work of reference, I will first give the views of the latest authorities in England, France, and Germany. I do not say they are the best, or the highest, authorities, but I think they will be accepted as fairly representing the faculty in their respective countries, and as exponents of the scientific and philosophical knowledge of their day.

In Reynolds's System of Medicine, published in 1866, there is an Article on Dysentery by Professor Maclean, from which I will take the liberty of extracting the History and Causes of the disease in full.

(*a.*) "**History.**—Dysentery was well known to the most ancient writers on medicine, and has largely occupied the attention of modern physicians, particularly of those who have served in fleets and armies. It has been seen in all climates, in the temperate as well as in the torrid zone. No country has been exempt from it; sometimes appearing alone, at others as a formidable complication of malarial fevers or scurvy, often treading in the footsteps of devastating wars; for in all ages it has been the scourge of armies, and one of the chief causes of mortality in unsanitary camps and garrisons.

(*b.*) "In the pre-sanitary age it was as common, and nearly as destructive to human life in Britain, as it is now in unhealthy tropical regions, as yet unvisited by the sanitary reformer.

(*c.*) "In Ireland, 'the looseness,' as it was called, was a common endemic disease, causing great mortality throughout the seventeenth century, and prevailing occasionally in an epidemic form, down to the year 1818.

(*d.*) "Dysentery has ceased to be a destructive disease in this kingdom; it has disappeared before a higher civilization, and what it brings in its train, viz., improved agriculture and drainage, more particularly subsoil drainage, the removal of filth from the vicinity of dwellings, the supply of purer water to our cities; in a word, increased attention to hygiene. Just in proportion as Malaria, the product of moisture and organic decomposition in soils, has been banished from our midst, so has dysentery ceased to be a prevalent and fatal disease.

(*e.*) "In India, among Europeans of all classes, this disease comes next to fevers in frequency, but the direct mortality caused by it is greater than from all the forms of fever known in that country.

(*f.*) "'Out of an aggregate British force of 25,433 men of Her Majesty's Army, serving in periods of eight and ten years respectively, in the stations of Calcutta, Chinsurah, and Berhampore, all in Bengal Proper, there occurred 8,499 cases of dysentery and diarrhœa. In the presidency of Madras, again, out of an aggregate British force of 82,342 men serving there from 1842 to 1848, there occurred 10,531 cases of dysentery, and 9,189 cases of diarrhœa, making a total of 19,720 cases of bowel disease, exclusive of cholera.'* Nor is this all, for most of the casualties which occur amongst sick soldiers on the voyage homewards from India, are from chronic dysentery.

(*g.*) "The Naval Medical Reports show that of late years, except on the East India, China, and West Coast of Africa Stations, bowel complaints do not cause a high mortality. In the Report for 1860, it is said that the 'intractable flux' of China was, as usual, far more destructive of health and life than any other disease that attacked the force. The death rate from this cause was in the ratio of 13·6, and the invaliding 25·9 per 1000 of mean strength.

(*h.*) "CAUSES.—When we consider the variety of causes to which this disease has been attributed, it is impossible to admit that agents so many and various can give rise to an affection which, in all climates, has presented so much uniformity in its symptoms and anatomical lesion.

(*i.*) "It appears to me that many of the so-called 'causes' of dysentery must be regarded more as active agents of *propagation* than of *causation*. For my own part, I believe dysentery to be caused by the action on the blood of a poison having a peculiar affinity for the glandular strictures of the large intestine. This poison I believe to be a malaria generated in the soil by the decomposition of organic matter. Once a common and fatal disease in this country, it is now so rare that a London hospital physician rarely, if ever, sees a case of genuine specific dysentery, save such as have been imported from malarious countries. How comes it that a disease with which our predecessors were so familiar has become so rare? Many of the commonly-received 'causes' are as much in operation now as then; *e. g.*, the combined action of cold and moisture, the action of irritants on the mucous membrane, unripe fruit, unwholesome and indigestible food of all kinds, feculent and other accumulations in the larger intestines, yet dysentery does not result. Is it not that, for the reasons already assigned, less malaria is evolved from the soil?

---

* Sir R. Martin.

(*j.*) "It seems that, just in proportion as we have banished malaria, so have we got rid of dysentery. For a long time the prisoners in the Penitentiary at Millbank were subject to visitations of dysentery at those seasons, and in those states of atmosphere, which most favour the decomposition of organic matter in the soil.

(*k*). "The late Dr. Baly, then physician to the prison, in the Gulstonian Lectures for 1847, has given a most instructive account of an outbreak of this kind. Dr. Baly has shown that the disease which prevailed in Millbank prison, was precisely the same in its symptoms, course, and lesions, as that described by Sydenham, and by writers on tropical diseases of the present time. He investigated its cause with much care, and was led to the conclusion that it was 'due to a poison introduced from without, viz., a malaria rising from the soil,' and that all the conditions required for its production abounded in the close vicinity of the prison.

(*l.*) "In India, dysentery prevails most, and is most fatal in, moist alluvial soils containing organic matter in a state of decomposition, as for example, very notably, in Calcutta.

(*m.*) "It is no doubt true, that the disease is sometimes seen, and that in a malignant form, in places which are not alluvial. I cannot give a better example of this than the old infantry barracks at Secunderabad, in the Deccan, of dysenteric notoriety; but there, as at Millbank, the conditions necessary for the production of malaria were only too abundant.

(*n*). "The sad but instructive history of those barracks has been given by Staff-Surgeon Crawford, and the writer of this article, in the Army Sanitary Report for 1860. The barracks stand (for, notwithstanding their dreadful history, they are still in use) on low ground, swampy on one side, and overshadowed by rocks on the other, exposed to the malarious influences of the marsh in the south-west monsoon, while the rocks on the other side shut out the invigorating north-east wind. A grave-yard, now closed, is placed close to the buildings, on a higher level, and in the direction of the natural drainage; another, at a greater distance, and in a less objectionable site, is on the south side. The surface and subsoil are thoroughly saturated with organic matter, the removal of which is impracticable. The neighbouring soil, until recently, when something has been done to fill them up, was furrowed by ravines, in which ordure was deposited by natives; the privy accommodation was of the worst possible construction, and badly placed; the barracks were invariably overcrowded, and surrounded by a high wall. Here were all the conditions for the production of *malaria*, and the causation and propagation of disease.

(*o.*) "For half a century the loss of life in these buildings, *chiefly from malignant dysentery*, was shocking. For some years it was nearly one in three of *strength;* so late as 1826 it was nearly one in every five. Between the years 1837 and 1858, out of an annual strength of 834·44 occupying those buildings, the admissions into hospital were 1529·40, and the deaths 37·20. In 1858 the 'Royals' occupied the barracks, with an average strength of 1,098; there were 2,497 admissions into hospital, and 104 deaths, being nearly ten per cent. of the strength.

(*p.*) "Here we have two examples, one in England, the other in India, of the local existence of malaria with a like result, the production of malignant dysentery; the symptoms and anatomical lesions being alike in both cases, with this exception, that in the barracks, hepatic complications were common, due probably to the greater intensity of the cause, combined with high temperature, and intemperance among the soldiers. It is quite true that the provisional term, malaria, is a vague one. In the present state of knowledge we cannot isolate it, and must here take it to mean a poison resulting from the decay of organic matter in the soil, which, when conveyed into the body, is capable of causing a disease, of which certain anatomical lesions of the great intestine are a characteristic and invariable incident.

(*q.*) "The following are usually named as 'exciting' causes, but, as already remarked, it is more than probable that some of them are merely active agents in propagating a disease that has a specific cause.

(*r.*) "IMPURE WATER.—'There is,' says Dr. Chevers, 'the strongest reason for believing that much of the cholera and dysentery which occur on board the vessels in the port of Calcutta, is caused by drinking the always muddy and filthy, and often brackish water taken up in buckets over the ship's side. Nearly every person, native or European, who comes to Calcutta suffers, more or less, just at first, from some kind of bowel complaint, but none suffer so much as seafaring men;' and, no wonder; for the same authority informs us, 'that opposite Calcutta the water is frightfully impure. There it receives some forty tons of excreta daily'—(and we may confidently assume that this enormous mass of impurity contains no inconsiderable proportion of cholera and dysentery stools, for these diseases are always present in that most unsanitary city)—'a multitude of bodies of dead cattle, and some 15,000 corpses yearly.' Dr. Rose makes precisely similar observations as regards seamen frequenting the port of Shanghai, in

China, and attributes the heavy mortality among them from dysentery to the same cause, viz., drinking river water loaded with organic impurity, and still further polluted by the excrements of an immense population.

(*s.*) "Miss Nightingale, in her Summary of the Indian Sanitary Report, observes, with too much truth, 'there is no reason to hope that any station (in India) has what in this country would be called a pure water-supply, and at some time it is to be feared that when men drink water, they drink cholera with it,' and we may add dysentery also.

(*t.*) "EXPOSURE TO COLD.—Dr. Mackay, R.N., describes the mode in which this cause operates among seamen serving in the malarious rivers of China. The men, when they lie down on the deck to sleep, pull up their frocks and coarse under flannel jackets, so as to expose the abdomen. When the cool night wind sets in, the exposed skin of the sleepers, from being bathed in perspiration, becomes dry and finally chilled, and in a very short time they awaken griped, and perhaps sick, and so commences very frequently an attack of what Dr. Mackay calls 'Sporadic Dysentery.'

(*u.*) "IMPURE AIR.—Dysentery once established is propagated by the effluvia from the evacuations of those affected. In most Indian barracks, a few years ago, the latrines were so badly constructed, so injudiciously placed, and so ill-kept, as to aid materially in propagating both dysentery and cholera, by exposing the healthy to the effluvia arising from the evacuations of those affected. I affirm, from frequent observation, that barrack-rooms most exposed to the effluvia of latrines always furnish the largest number of dysenteric cases, and the heaviest mortality.

(*v.*) "In like manner I have seen the disease propagated in hospitals by the practice of preserving the evacuations of large numbers of dysenteric patients, for the inspection of medical officers at morning and evening visit. No single measure of a preventive kind yet tried has exercised a more beneficial effect on the health of troops in India, than the improvement which has been introduced in the position, construction, and conservancy of barrack and hospital latrines."

Trousseau delivers his views upon dysentery in the colloquial form, which precludes his treating it in the systematic manner employed by writers generally. I will, therefore, take such extracts from his Lecture (LXXIV. New Sydenham Society, 1871,) as bear upon the points herein discussed.

(*a.*) "Gentlemen:—The year 1859 will be looked back to as remarkable for the frightful epidemic of dysentery which we have just traversed. The disease has prevailed throughout all France in a more general manner than on the occasion of previous dysenteric outbreaks; and it has not spared Paris, where for the last hundred years isolated cases only have occurred. The epidemic, exhibiting its usual features, declared itself about the end of July: it attained its maximum severity in September; and though it continued during November and December, it was much less prevalent.

(*b.*) "Of all epidemic diseases, dysentery is certainly the most severe and the most deadly. Outbreaks of dothinenteria, scarlatina, small-pox, diphtheria, and even cholera-morbus itself, carry off fewer victims. Desgenettes states that dysentery killed a greater number of our soldiers between 1792 and 1815 than fell in the great battles of the Empire. This we can understand, for dysentery is not only very deadly, but it breaks out as an epidemic much more frequently than other diseases, and invades particular regions at very short intervals.

(*c.*) What are the *causes* of epidemic dysentery? The causes of this, as of most other epidemics, elude our observation; though the inquiry has been pursued very carefully, nothing positive has yet been established in respect of the conditions in which it originates.

(*d.*) "In Tours there are two barracks, one in the eastern and the other in the western faubourg: they are similarly situated, and at an equal distance from the river which flows through the town. The same hygienical system is adopted in both; and in both also the dietary of the soldiers is exactly similar. Nevertheless, during the twenty years which preceded, and the ten years which followed, the period during which I studied at Tours, it was always in the cavalry barracks that the disease first broke out. The few soldiers belonging to infantry regiments who were seized with dysentery at the beginning of the epidemic had contracted it in hospital, whither they had been sent for other diseases; and it was not until a later period that the epidemic showed itself in the infantry barracks.

(*e.*) "Here, then, is a case in which no charge can be brought against the local situation, the hygienical conditions, or the food. You are aware that it is very common to impute the causation of dysentery to the use of fruits: so general is this opinion, that one finds it rather difficult not to acquiesce in it. It is, however, a prejudice against which the greatest practitioners of former times have contended. Without going back to Alexander of Pralles, who taught that grapes and other fruit not only did not produce dysentery, but were, on the contrary, really preventive, and very often curative, I shall lay before you the views on this subject of Stoll and Zimmerman, two of the most illustrious physicians of last century."

(They refer to the exploded doctrine of fruit as causative of dysentery, and may be omitted.)

(*f.*) "It cannot be denied that the spread of the disease is promoted by unfavourable hygienical conditions, such as hot weather, bad food, and crowding; but they are only proximate causes, to which we must add another something, and that something we call the *epidemic constitution.*

(*g.*) "We cannot otherwise explain why dysentery does not always show itself in those years in which the heat is greatest;

why it does not invariably appear where there is overcrowding; and why, for example (not to go beyond this line of argument), it so generally spares Paris, so little spared by other epidemic diseases. Therefore, as I have just been saying, we are in absolute ignorance of its primary cause."

(*h.*) "We know, however, that when once developed, it is exceedingly contagious; although Stoll denies the contagious character of dysentery, as well as of scarlatina. That both diseases, however, are contagious is evident. In small places, it is easier than in great centres of population to trace back the disease to its source, and to follow its progress in the regions which it invades. Have not our honourable colleagues of the army of Africa, where dysentery, at intervals, commits great ravages, told us that when it prevails in a regiment, it declares itself at every station where that regiment halts, thus following in the march of our expeditionary columns. And when, from the Algerian hospitals being overcrowded, some of the dysenteric patients have been sent to Marseilles, that town has become the centre of an epidemic such as had never occurred before the arrival of these sick soldiers."

The German writer I select is Professor Niemeyer. In the Chapter of his Text-Book (translated by Drs. Humphreys and Hackley, 1870) on Dysentery, occur the following passages:

(*a.*) "Dysentery poison cannot be directly observed, as an organic, living substance, any more than the poisons inducing other infectious diseases can; but the reasons so often repeated, especially when speaking of typhus, induce us to refer dysentery also to an infection of the body by a certain species of low vegetable organism, and to speak of a 'dysentery germ' as we have already spoken of a 'typhus germ' and a 'cholera germ.' From this point of view, we may, to some extent, understand the facts which have been determined by thorough observation concerning the spread of this disease.

(*b.*) "Dysentery results, although not exclusively, from a miasm; or, in other words, the dysentery germ grows, flourishes, and increases outside of the human body; and persons staying near its locality are in danger of being attacked by it. The circumstances favourable to the increase and propagation of the dysentery poison, among which a high temperature and a certain amount of moisture are prominent, exist in the tropical regions; there the disease is endemic through large portions of country. According to the classical work of *Hirsch*, in Europe only the peninsulas, as the south of the continent, and the islands about them, constantly offer such favourable conditions for the increase of the dysentery germ as to cause the disease to be endemic there. But, through almost all Europe, the conditions for the increase and propagation of dysentery, which is endemic with us also, are occasionally so favourable, especially late in the summer, that the disease becomes epidemic. The circumstance that dysentery is not endemic or epidemic in all regions where high temperature and moisture constantly exist, justifies the conclusion that these are not the only things necessary for the growth of the germ, or else that it is not so widely spread as to be found everywhere that conditions favourable to its development exist. The coincident epidemic or endemic occurrence of dysentery and intermittent is frequent, but not at all constant, according to the recent observations of *Hirsch*. Dysentery exists where the requirements for malaria—marshes, etc.—are not present. It attacks the open country oftener than the city.

(*c.*) "The dysentery germ appears to reproduce itself always, or under favourable circumstances, in the body of the infected person, and it would seem that the dejections of the patient contain the contagion thus formed, or its components; for, while it has not been proved that one person catches dysentery from another, it is more than probable that the disease may be communicated to healthy persons through the dejections of dysentery patients, or by the night-stools, bed-pans, or enema syringes that have been used by them. This causes dysentery to resemble cholera, while it speaks against the asserted connection between it and malaria. Why should not the same or similar influences, such as high temperature and moisture, favour the development of various specific low organisms, just as it does the increase of different varieties of higher plants and animals?

(*d.*) "Catching cold, getting wet, great fatigue, the use of unripe vegetables, and other injurious influences, have been advanced as causes of dysentery, and it cannot be denied that persons exposed to these influences are more readily affected than others. Nevertheless, infection with a specific poison is the sole cause of this disease, and the part that the above influences play in the etiology is only to render the organism more sensitive to the action of the poison; in other words, they increase the predisposition to dysentery."

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3. I regret that I have not yet been enabled to procure such works on dysentery as may have come of late from other nations—more especially Italy and America. I apprehend, however, that the discovery of the source of the dysenteric poison would have been rapidly disseminated through the civilised world, and that, therefore, nothing material has been done in the way of ascertaining its origin. Rome, Naples, and Genoa, I gather, are still subject to the disease; and the results of the war between the North and South, go to show that, at that time, the key to the mystery of dysentery had not been found by the Americans.

4. While engaged upon another subject of a purely local nature—a little popular treatise connected with the regimen of Victorian colonists—I had occasion to look into the question of climatic influence in its relation to dysentery. It was necessary, for the purpose I had in hand, to determine, if possible, how far the peculiarities of this latitude, as regards atmospherical, electrical,

and other conditions, were concerned in the causation of this disease. With this object I turned to Reynolds's System of Medicine, where I found the article of Professor Maclean. As the matter was only collateral to my main object at the time, I accepted the alluvial theory, without much thought. I adopted Professor Maclean's views, and set about incorporating them. I had got as far as pointing out the necessity for careful attention to associated hygiene in communities, especially in warm climates, as well as to individual regimen in foreign regions. And I was just about to point the moral by predicting an awful pestilence of malignant dysentery in Melbourne some day, by reason of its undrained and unsewered state, when I was suddenly brought up by the reflection that Melbourne was over-ripe for dysentery at that moment, and had been, moreover, for years. If the cause of the disease was that given, and malaria from the soil was the source of the specific poison, Melbourne never presented more perfect conditions, or in so great abundance, for its evolution.

5. Then, too, the severe epidemic of malignant dysentery that ravaged the city from 1853 to 1855 occurred to me. How was it that that epidemic wore itself out—the disease getting gradually fainter after the second or third autumn, and steadily declining up to the present time, though many—too many—isolated cases of acute and fatal dysentery are registered still? How was it the disease had never returned with its original virulence? If the sewerage and drainage of Melbourne had been carried out perfectly in the meantime, of course the explanation would have suggested itself easily enough. That work would undoubtedly have received the credit of ridding us of this most deadly of all plagues. But in the absence of that ready mode of accounting for the reduction of the malady, it was difficult to reconcile the decrease of the disease with the increase of its cause. It was a startling anomaly to find that, while the saturation of the soil in and around Melbourne with decomposing organic matters was progressive and cumulative, the disappearance of epidemic dysentery was rapid, complete, and apparently final. It was so contrary to the theoretical explanation of the causation—indeed, from that standpoint, it was paradoxical and absurd—that it was evident there was some fallacy somewhere, and that the cause of dysentery had not been arrived at, so far as Melbourne was concerned.

6. Then again I threw my memory back to the still more severe and fatal epidemics of dysentery that had broken out on all the gold-fields of this colony, in the early days. And the subject was involved in yet greater obscurity. For I was at once confronted with the fact that here malaria was not only not a presumable or probable cause, but that it must actually be excluded as even a possible cause, of the production of the dysenteric poison. There was neither natural, nor artificial, malaria, from decomposing organic matters in the ground. There could not have been anything of the kind in localities where there was no alluvial soil, and where the surface had been occupied only a

month or two. Defective drainage, or sewerage, was out of the question therefore. And as for malarial influence in the bush of this colony, the thing was not to be entertained for a moment in a country where a case of ague, or other paludal disease, was never heard of as an indigenous affection.

7. Besides, there was another significant fact which precluded the idea of assigning this particular malaria as the source of the poison. The gold-fields are there still, and the people are there still, in even larger numbers. They work in the same way, and are subject to the same local influences. And yet, as in Melbourne, so on the diggings, two or three years sufficed for the extinction of epidemic dysentery—except when the factors of the poison have been set to work anew, and the previous conditions have been brought into operation again, as at new fields, or "rushes," as they are called. No sooner was anything like permanency arrived at on any spot, and a more settled state of affairs obtained, than dysentery disappeared unaccountably. For, on the assumption of alluvial malaria, it ought to have been precisely the other way. But when the people clustered together, and converted their encampments into virtual townships before the passing of Municipal Acts, they had all the drawbacks, without any of the advantages, of local self-government, in the matter of hygiene. There were no means of dealing with garbage, or sewage, or refuse matters of any kind, effectively. The consequence was, the collection here and there of abundant decomposing organic matter, in the midst of a teeming population. Still, in the very face of this, the specific or zymotic poison of dysentery was not generated in larger quantities, but actually died away.

8. That there was a fatal flaw somewhere in Professor Maclean's reasoning, I was now convinced. I therefore examined his facts and arguments more critically and analytically, and the conclusion was forced upon me that the facts not only did not support, but were subversive of, the malarial theory. For on tracing back the history of the disease in London, as I had done in Melbourne, I brought out precisely the same result as to the disappearance of dysentery from the two places, viz.:—That dysenteric epidemics did not disappear from either city in consequence of effective sanitary arrangements, but in spite of the most defective hygienic machinery, and of a constantly increasing saturation of the soil with impurities of all kinds. While the disease in London was steadily on the decline, the alluvial pollution of that city was rapidly increasing; and by the time it had reached its maximum—that is, when sanitary measures were first brought into play—malignant epidemic dysentery had been unknown for a century.

9. The decadence of the "looseness" of Ireland is hardly to be assigned to the introduction of improvements after 1818; for it may be questioned whether, even at the present day, a tenth part of the island has been brought under such conditions as are assumed to explain the extinction of epidemic dysentery. In Scotland, possibly, some apparent interdependence between drainage and the

disease might have been traced; for there, cultivation on improved principles followed the scientific agricultural movement in England more closely. But in none of the countries, as it seems to me, can subsoil drainage, or agricultural improvements, be shown to have had any connection with the decrease of dysentery.

10. In reflecting upon dysentery as the proverbial scourge of armies, the alluvial malarial view appears to break down. For though it is undoubtedly true that troops have been frequently exposed to the influences of malaria, still it cannot be supposed possible that every position they have occupied, every environ of every town they have beleagured, since fighting began, has been unhealthy from exhalations from the soil. Generals must now and then have pitched, if only by accident or from necessity, on salubrious ground, where the invariable plague has nevertheless assailed their forces. Besides, the sites of many battles and sieges are well known. There are probably some on which dysentery neither preceded nor followed the army. But to establish alluvial malaria as a causation, it would have to be shown that in every instance dysentery was a topographical necessity. To take the case of England herself. Can it be demonstrated that, on any given spot where a force was encamped on English soil, the dysentery with which it was (of course) affected, has remained ever since—or did remain until drainage and sewerage removed it? And yet the alluvial malarial theory exacts this test.

The whole history of camp life, however, in all ages and in all parts of the world, appears to me to point irresistibly to the conclusion, that the cause of its accompanying dysentery is not to be searched for in the ground selected, but in the conditions created. The encampment does not find the cause where it settles, but carries it there, or makes it there. And there seems to my mind the closest analogy in this respect between the surroundings, or conditions, of a siege, and those of a "rush" on a new gold-field. In both, the resultant, dysentery, appears to be entirely unconnected with local malaria. Malaria may be coincidental and accidental, but is neither invarible, nor essential. It may modify the form, aggravate the symptoms, and promote the *propagation* of dysentery, but the *causation* of the specific dysenteric poison is clearly not dependent on malaria.

11. In India—where he has evidently had great experience of the disease itself, and has worked hard, and successfully, to lessen the frightful mortality from dysentery—Professor Maclean seems to have failed in bringing out its causation. He frankly admits that the alluvial reasons which apply to Calcutta do not hold good in the Deccan. In fine, not to elaborate further, I was so convinced of the insufficiency of alluvial malaria, that I went further afield.

12. Trousseau did not enlighten me much, and the confession that the world was in "absolute ignorance of its primary cause" was not encouraging. Nor could I find anything more definite anywhere else. Yet I was now fairly fascinated by the subject,

and though it looked hopeless to expect to throw any light upon a matter that had engaged the attention and baffled the enquiry of men in all ages, I nevertheless continued to speculate. The problem was always before me. I hammered at it until I had narrowed down the possible solutions to very few indeed, when an explanation occurred to me of so exceedingly simple a nature that I was almost inclined to discard it at once, on the ground of its being so very evident that it could not have been missed by enquirers. It seemed almost a waste of time to follow up the train of thought started, because it seemed too patent not to have impressed itself upon the notice of those who had specially devoted themselves to the investigation of the causes of dysentery. Yet, adhering closely and faithfully to the exhaustive method I had proposed to myself, I half reluctantly began what I felt convinced could only be a work of demolition and supererogation. I had not gone very far, however, when I was surprised to find that this, seemingly, too simple explanation stood several of the tests which had sufficed to dispose of the others. I noticed, too, that, strangely enough, none of the authors whose works I had consulted had entertained the subject at all; and I then began to suspect it was one of those very obvious things which every one believes that some one else has, at some time or other, taken up and dropped, and is therefore not worth consideration. Whatever may have been the reason, however, for the omission, I could not find that any enquirer had ever entered upon this special line of investigation. I searched for anything of the kind that might have been done, in vain; and though my opportunities in this direction were limited, I think the sequel will show that I was warranted in coming to the conclusion that the particular question I was then engaged upon had never been mooted; or if it had, it had not, at all events, been thoroughly sifted.

13. After much patient thought, and the application of such severe and rigid tests as occurred to me, and as could be applied with the means at my disposal, I finally arrived at what I believe to be the solution of the problem of the cause of dysentery. Whether that solution, however, will stand the severer and more rigid tests which will now be applied;—whether it will survive more extended observation and more learned and searching investigation, and whether it will successfully undergo the ordeal of actual experiment and future experience—must be left. At the present moment, I can only say that the further I have gone since the idea first occurred to me, the more thoroughly confirmed I have become in the soundness of the views herein submitted. I have been unable to find, after diligent search, one single fact that militates against them. I must, therefore, leave it to others to demolish, or to accept and establish them, as the case may be.

14. Before proceeding to advance the theory of the causation of dysentery, it may, perhaps, be permitted to explain the mode by which I arrived at what might appear, at first blush, to be a crude, wild, fanciful, extravagant, or ridiculous notion. This will clear

the ground and enable the reader the better to appreciate the nature and scope of the enquiry I ventured upon, and also to determine for himself whether the proofs to which I submitted my own deductions are sufficient or not. In doing this it will be understood that the successive steps are not given in the exact order in which they were taken. For the object is merely to show that the ratiocination, though perhaps imperfect, had at least some method; and that the conclusions, if unsound, have not been jumped at, or hastily admitted.

14. After a few preliminary failures in the attempt to get at the factors of the specific poison of dysentery, and after reading divers accounts of the disease, I found it necessary to go to work more systematically. I soon settled down to the conviction that, whatever its cause was, it must be one that had been omnipresent in all ages, and common to all countries (excepting, perhaps, the frigid zone), and that it was still universally active everywhere, or might be called into activity anywhere, in every region of the earth—not excluding Great Britain even. For not only are sporadic cases met with in England still, but dysentery has appeared in the epidemic form in the northern counties (Bolton and elsewhere) within the last 30 or 40 years, and perhaps later. Dr. Aitken observes—

"In England generally, however, dysentery, as a cause of death, has been decreasing since 1852, although about 200 years ago it was one of the most prevalent and fatal diseases of London. Yet still, although the disease is less violent and less fatal (for as a cause of *death* it has remarkably diminished during the past ten or twelve years), and although the unfavourable hygienic conditions which were wont to bring about dysentery no longer exist, the active endemic conditions which favour, promote, or are congenial to its development, are only dormant, and not eradicated. The disease, therefore, is still sometimes brought about just as in the days of Sydenham or Willis. In no respect, however, do we find that the dysentery of this time differs essentially from the description given by Sydenham more than a hundred and thirty years ago."*

15. Even in England, then, dysentery has been only scotched, not killed. This universality of the disease implies an equal catholicity of its cause. It was evident, therefore, that any hypothesis, or theory, of causation, which should not be applicable to all times and all places, must be ruthlessly set aside. No matter how plausible, or ingenious, or apparently sound, as regards one region at any given period, it must inevitably be fallacious and unsound if it would not apply to the same region at another period, and to all other regions at all other periods [*i.e.*, assuming the specific nature of the dysenteric poison]. Unless a proposition on this subject could bear this proof, it was clearly worthless, and might

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* As a matter of fact Sydenham died in 1689, and he wrote "Of the Dysentery of part of the year 1669, and of the years 1670, 1671, 1672."

be rejected at once. For there can be no exceptions to natural laws.

16. Provided with this ready touchstone, one could make short work of most of the presumed, or assumed, causes of dysentery. Alluvial malaria had already been dealt with, and eliminated. Bearing in mind the useful practical hint conveyed in the words of Trousseau—"In small places it is easier than in great centres of population to trace back the disease to its source, and to follow its progress in the regions which it invades"—I tried many of the simpler suggestions that have appeared from time to time, as well as the more complicated theories extant, by the test of New Holland. If the suggestion or theory did not fit exactly to the endemic dysentery of this colony of Victoria, it was thrown out at once. And by this efficacious means I rapidly got rid of volcanic agency (Batavia and South America); particular electrical conditions of the earth and its atmosphere; ozonic changes; the larvæ of insects (W. Indies); exhaustion and other deleterious results from heat, cold, moisture, exposure, crowding, ventilation, mal-nutrition, denutrition, innutrition, and all errors in nutrition (whether from salt provisions, putrid or rancid meat, crude or unripe vegetable matters, as pickles, stone fruit, and every vegetable esculent known; or from scurvy, or any other disease). These various local and general causes to which the disease has been attributed, taken either singly or in any form of combination or arrangement that suggested itself, collapsed at once at the touch of this Australian proof, and were dismissed without further thought. But among the alleged causes of dysentery, I fell across one which rather perplexed me. It is contained in the following paragraph from the "Biennial Retrospect of the New Sydenham Society for 1871"—page 175:—

"Dyes (*Journ. f. Kinderkr.*,' 1870, liv. 317) observes that while some autumns pass without any epidemic of dysentery, diarrhœa (simple intestinal catarrh) may be very prevalent. He considers that the former complaint is due, not to the unripe fruit, which merely sets up diarrhœa, but to a peculiar viscous pellicle (smut). In some years, more than in others, it covers the fruit, more especially plums, and, according to him, is composed of animal parasites. He holds (p. 321) that the mildew of the fruit, which sets up dysentery in man, has a very great resemblance to that of the fodder which in cattle causes the foot-and-mouth disease in all its varying forms. To oppose its effects in men, chlorine water should be administered to destroy the parasites; and this treatment, combined with one promoting skin-action, prevents altogether the dysentery which would be provoked by such fruit."

Now, although there was a tone of authority and an appearance of research about this, I nevertheless ventured on submitting it to the test of universality; and, on finding it wanting, I had no hesitation in summarily rejecting this view of the parasitical causation of dysentery. Of course, it will be understood that I do not question the facts, or microscopical observations; but I challenge the deduction from them. The mildew may be on the fruit; that mildew may consist of certain easily distinguishable animal parasites; those parasites may be readily found in persons who have partaken of the fruit; and those persons may have dysentery. All this may be granted, and yet it is an inconsequence that the dysentery is caused by the animal parasites. It certainly does not follow of necessity. In fact, as the extract stands, there is a decided severance—*longum intervallum*—between cause and effect. In the absence of the detailed views of this observer, it is impossible to deal satisfactorily with the theory he has broached. For all one knows, he may have assailed and upset the "dysentery germ" of his learned countryman, Professor Niemeyer, and may have thrown down the generally received doctrines as to specific poisons. He may have controverted the dictum that a given specific poison can be produced by one factor, or one set of factors, only. He may have denied even the existence of these poisons; and may have gainsaid the notion that cholera, dysentery, yellow fever, the exanthemata, miasmatic diseases, and the like affections, can be caused by their several specific poisons only. As nothing of the kind, however, appears in the *précis* of his labours, it is highly improbable that Dr. Dyes has gone to such extreme lengths as to try conclusions, on these points, with the now generally accepted views of the highest schools of medical philosophy. If he had, there would surely have been some notice of it. Oue is forced back to the conviction, therefore, that this writer still holds to the established postulates on these matters. And this it is which makes it difficult to reconcile the animal parasite view of causation with the recognised views of specific poisons. As the matter now stands, Dr. Dyes appears to have committed himself to this proposition, viz.: Animal parasites have been the one cause of dysentery in every country since the world began. I do not know whether he intended to enunciate this view, in stating the results of his investigations; but it flows naturally from the premises, and is not strained in the least. For, if he had succeeded in showing clearly that smut, under certain conditions, had once caused dysentery, it would follow (by the law of specific poisons) that smut has always caused dysentery whenever those conditions have been present; that it will always cause dysentery whenever those conditions shall be present; and that nothing else whatever could ever have caused dysentery in the past, or can possibly cause dysentery in the future, but smut under the same conditions.

17. Possibly, Dr. Dyes takes this view. If so, it appears difficult to reconcile it with the well known prevalence of dysentery in so many regions, under circumstances where the supposition of stone-fruit, or any other fruit liable to be affected with a similar smut, can hardly be entertained. One instance in which the viscous pellicle from unripe fruit could not possibly have been concerned in the causation of dysentery, is sufficient to show that it never could have been concerned in any instance. Not to quote Aus-

tralian cases, where fruit of all kinds was unknown when dysentery was epidemic, and is common and abundant now that epidemics have ceased, I will submit an instance of the occurrence of this disease, in which animal parasites upon any description of fruit are altogether excluded. If he can trace the intervention of animal parasites from any form of vegetable smut—not alone unripe fruit—in this particular case—of course, on such a scale as to account for the attack—Dr. Dyes will show that these minute organisms play a far more important part in the causation of the "dysentery germ" than they would appear to play at this moment. However, let him examine his theory by the dry light of the following account. I cite it here because it not only assists in clearing away animal parasites, but it thrusts many other notions as to the sources of dysentery into the background. It is also remarkably illustrative of the peculiar view of dysentery to be hereafter submitted. Indeed, it is one of the most unique cases on record; and affords the strongest presumptive proof and the clearest negative evidence of the correctness of that view.

In the *Edinburgh Medical Journal* for 1810, there is "An Account of the Epidemic Dysentery which prevailed among the Dutch troops at the Cape of Good Hope, in 1804 and 1805; with Remarks, &c. By Dr. Henry Lichtenstein of Helmstadt;" from which I take these extracts:—

"The Governor of the Cape, having been informed that an expedition against that colony had been prepared in England, collected all his troops, amounting to about two thousand five hundred men, towards the end of September, 1804, in a camp, from whence he could readily lead them to all the points where an attack was practicable. This camp was stationed to the eastward of a mountain, in a sandy plain, to which it was requisite to fetch all the water from a considerable distance: it was also exposed to the concentrated rays of a burning African sun, from its rising until within an hour of its setting, from which the soldiers, overcome with the heat, and oppressed with fatigue from their necessary exercises, had no other shelter than their tents. Yet no sooner had the sun disappeared below the horizon, than the sea breezes, of which not a breath had previously agitated the heated atmosphere, immediately swept off as it were the heat of the plain; so that the thermometer [of Fahrenheit] which was sometimes elevated during the day to 90 degrees, would at once fall 25 or 30 degrees. Here was obviously sufficient cause of disease: and before the end of October, 1804, dysentery showed itself in the army, which in other respects was well ordered and supplied. About the same time an epidemic catarrhal fever appeared among the men, which affected both the abdominal viscera and the organs of respiration, and which, as it originated from the same causes as the dysentery, differed only by the absence of the intestinal flux. At the end of December four hundred and eighty-eight men had been attacked by these two diseases, of whom ninety-one had died. The camp was then removed to the vineyards, nearer the mountain, and behind a wood which sheltered it from the wind. This removal, together with the favourable change of season, produced a considerable diminution of the number of the sick: for during the first three months of the year 1805, only a hundred and forty-nine were received into the hospital; and, thanks to the new mode of treatment from that time adopted, only fifteen of this number died. The distressful sequelæ of the disease proved fatal to twenty-seven of those who were attacked before the beginning of 1805; so that of six hundred and thirty-seven patients, one hundred and thirty-three died—a proportion somewhat more than one in five. By the end of March the epidemic had ceased.

"The author of this memoir, then chief surgeon of the battalion of Hottentot light infantry, was not ignorant that epidemic dysentery is also the scourge of armies in Europe, where it is distinguished, according to the variety of its forms, into catarrhal, bilious, nervous, &c.

"The dysentery commonly began its attack suddenly, but with so much seeming mildness, that the majority of those affected paid little attention to it during the first two or three days.

"The catarrhal bilious fever, which made its appearance at the same time with the dysentery, differed from the latter only by the absence of the bloody diarrhœa.

"At the end of November five or six patients died every day, and every day the camp sent more than double that number to the hospitals of the town.

"Matters became every day more critical, especially as the fear of an attack from the English had not ceased. * * * In consequence of the distance of the camp, and the uncertainty of the early symptoms, it happened that the patients were scarcely ever received into the hospital until the second day of the disease. * * * The patients, who died before the ninth day, retained their senses perfect, and had no delirium. Although the disease generally made its attack suddenly, and without any previous indisposition; it was sometimes preceded by, &c.

"None of the European troops suffered so severely from this epidemic as the fourth battalion, consisting chiefly of Germans, many of whom had suffered an intermittent fever, some years before, when in garrison in Zealand; nearly one-third of the sick belonged to this corps. The other battalions, which were composed of men, of various nations, but principally of Poles, suffered much less. Scarcely any of the cavalry were affected with the epidemic; having better pay, their fare was more nutritious; their exercises, besides, were less fatiguing; they were employed in dressing their horses at the very time when the chill of the evening came on; and they were possessed of cloaks, which the infantry had not. The volunteer cavalry, consisting of robust colonists, inured to fatigue by their labours in the field, likewise suffered little. But, what is remarkable, the Hottentots, a sort of savages, accustomed to the climate, suffered much in the army, although better clothed, better fed, and more cleanly than in their agricultural occupations; while those who remained in the employment of the colonists, exposed to all the unwholesome weather, undergoing much fatigue, and being badly fed, were much less affected by the epidemic."

18. It appears to me that this attack of dysentery, after a month's encampment on that *sandy plain*, shuts out Dr. Dyes's view of the origin of the disease. For it seems impossible to conceive how animal parasites could have been introduced to such an extent as to have caused this affection. Nothing is said by Dr. Lichtenstein about grapes as a cause; and he would have been morally certain to have alluded to this fruit, if it had been brought into camp in abundance. My impression is that the grape season is over at the Cape by October; though on this point I have no accurate knowledge. I should not have raised the question of grapes at all, but for the mention of vineyards by Dr. Lichtenstein. But since it has turned up in this way it may as well be disposed of. The grounds upon which I rest my objections to mildewed grapes, are:—(1) they are not spoken of by Dr. Lichtenstein; (2) the distance of the camp from the vineyard, which, though not definitely given, was probably many miles; (3) the grape season was over (?); (4) the Hottentots in the neighbourhood of the vineyards ("in the employment of the colonists exposed," etc.) "were much less affected by the epidemic," than the Hottentots in the army; (5) grapes do not cause dysentery in any part of Africa, at the present time, or are not supposed to do so. They are not mentioned as a cause there. In fact, dysentery appears to be unknown now at Cape Town. In "the Cape and its People," a volume of Essays edited by Professor Noble, there is an Article on "Our Climate," by W. H. Ross, M.D., in which he enumerates the diseases, and the word dysentery does not once occur. Dr. Ross gives a table of the mortality of the British troops, from 1822 to 1834. Under the head of "Diseases of Stomach and Bowels," I find there were 584 admissions and 15 deaths—among the whole force in 12 years. In Dr. Parkes's Practical Hygiene (p. 540) it is stated —"garrison 4000 to 6000 men, chiefly Europeans." * * * "Malarious diseases are very uncommon. 'Continued fevers' (probably typhoid) are seen, and are rather common, though not very fatal." * * * "In the earlier periods dysentery and diarrhœa were very common; they are now less so; in many cases, especially in the small frontier stations, they were clearly owing to bad water."

19. At these frontier stations there were probably no grapes, while in the vicinity of Cape Town, where dysentery has disappeared, the cultivation of the vine has been steadily increasing. All things considered, therefore, the evidence is strongly against the view that mildew from grapes could have brought about the endemic dysentery among the Dutch in 1805. And if that mode of conveying animal parasites into the economy of the troops be taken away, it will be extremely difficult to see where another can

be found by any stretch of ingenuity—unless, perhaps, in the atmosphere. The prevailing winds, however, in this instance, were from the coast. But I will not pursue this shadow of the causation of dysentery further. Indeed, if it had come in the pre-microscopical period of research, it would not have received a moment's consideration. But appearing as it does among modern medical literature, it had to be dealt with more carefully. I would observe that my remarks on Dr. Dyes's views have been made on the supposition that they are correctly summed up in the quotation given. And I would add that it has been far from my intention to appear to undervalue microscopical investigations. I have a clear sense of their importance, and a keen perception of the fact that on the Microscope and on Organic Chemistry, we shall probably have to depend for the eventual actual demonstration of the specific poisons. Indeed, the aid of the microscopist will have to be invoked to assist in determining the soundness, or unsoundness of the theory herein hazarded as to the factors of one of these poisons. As will be seen hereafter, I hope to utilise the discovery of another kind of parasite made by another German microscopist —Hallier. But while one may appreciate to the full the value of the microscope, there is no occasion to resign one's judgment to all the views of the microscopist. He may make his observations carefully and describe them faithfully, and yet his conclusions may be narrow or unsound. And it would certainly appear that the only thoroughly sound and unquestionable deduction arrived at by Dr. Dyes, is contained in the concluding sentence of the paragraph quoted. There can be no reasonable doubt that the remedies he suggests to counteract the effects of plums, will prevent "altogether the dysentery which would be provoked by such fruit."

20. To recur to the Dutch troops. It will no doubt have been observed that this occurrence of dysentery on the sandy plain at some distance from the mountain, disposes just as completely of miasmatic, or paludal, or alluvial malarial, causation, as the endemic dysentery of the Australian gold-fields. But besides this, it enables one to strike out many others. For instance, there was no exhaustion, or mental depression, such as occurs in an army after disaster, or demoralisation, of any kind. Neither was there any want, or anything to complain of, in the food. For the force was "in other respects well ordered and supplied." The Hottentots even were well fed — better in fact than the other natives who suffered less from dysentery. [These latter, probably, came from the stations, or homesteads, of the neighbouring boers with provisions.] On the whole, it may reasonably be assumed from the account, that the Dutch army was in tolerably fair plight at the time when dysentery suddenly appeared—at the end of the first month. So that all possible causes of the disease usually given, would appear to have become narrowed down to two; viz., atmospheric changes, and water. For both these have the one great essential requirement which it has been shown every theory of its causation must necessarily have. Whenever and wherever

dysentery has been, air and water have been. Therefore they fulfil one condition. It now remains to consider how far they answer in all other respects.

21. ATMOSPHERIC CHANGE.—The alternations of heat and cold have long been reckoned among the *predisposing causes* of dysentery. There are very few accounts of its epidemics in which extreme changes of temperature are not recorded. Hot days and cold nights, with occasional showers, or heavy dews, are almost invariably mentioned. But no writer has ever gone so far as to say that uncontaminated air, of itself, whatever successive changes it may undergo, and whatever may be its hygrometric state, can produce dysentery. Nobody has ever advanced the opinion that the air is the sole factor of the dysentery germ. In fact it is too palpably insufficient, as the slightest reflection will show. One has only to recollect that bushmen in Australia—drovers especially—sleep on the bare ground, and live entirely in the open air for twelve and fifteen months together, without getting a touch of the disease. Even at the very time when dysentery was epidemic at every gold-field, stockmen were travelling with cattle and camping out all round the diggings, and yet never became affected unless they went on the diggings themselves. Therefore atmospheric changes will not produce the specific poison. We come then to—

22. WATER.—That this element when used for drinking purposes may be largely causative of dysentery there is abundant evidence to prove. This has been established by a chain of evidence too strong to be snapped. The authorities cited on this point by Dr. Parkes, Professor Maclean, Dr. Aitkin, Dr. Copland, and others, leave no possible room for doubt. The "bad water" theory, therefore, is not to be shaken. The effect follows on the cause too closely to admit of squeezing in the slightest question as to their intimate and direct relation. We have now, therefore, traced the causation of dysentery to its source, and have got its germ at last. But yet the enquiry is not over. Although we know where the specific poison is, we have yet to learn what it is.

23. Some important points will now have to be considered, in order to determine what the water contains, and how it gets there. (*a*.) Is rain-water capable, of itself, of generating the poison? (*b*.) Can spring-waters, or storm-waters, collected in natural reservoirs, or flowing in ordinary channels, of themselves, furnish the elements which, under certain conditions, may be converted into the morbific germ—without the intervention of man? That is to say:—Would it be possible, in any country, for water to acquire the noxious elements for the formation of the poison, either by taking up salts, or other soluble material (becoming brackish), or by sweeping off the surface organic matters in a state of decay, or prone to decomposition—supposing the country were uninhabited? In other words:—Is the poison a natural product of, or an artificial introduction into, water? (*c*.) Is water essential to the production of dysentery? Can the poison be evolved without intermix-

ture with water? Must the germ be invariably introduced into the system by means of its solution or suspension in water? (d.) What are the conditions and factors required to convert wholesome water into "bad," or dysenteric, water? Or what is the cause of the empoisonment?

24. There are many other collateral and cognate questions arising out of the subject, chiefly dependent upon the nature of the answers to these; but they may be left for the present. (a) Rain water may be dismissed at once. It is obviously as incapable of producing the poison, of itself, as atmospheric air, of itself.* (b) The evidence is strongly in favour of the conclusion that "bad water" has been polluted by the agency of man, and that it is never found where this cannot be clearly traced. Instances are common where an army has advanced some distance into a country without the occurrence of dysentery, and yet, on returning by the same line, the troops have become affected by this and other camp diseases. On the assumption that the flux has been produced by the water drunk, it would seem unlikely that this has become vitiated in the meanwhile by natural means. It is a well known fact that detachments on the march, or rapidly moving columns, in sparsely populated regions, do not get dysentery (except in India, and in countries where the germs of the disease are now never absent). It is only when they halt for a period that they are liable to become affected. But there is a still stronger argument. When large masses of men are moved into regions where dysentery does not then exist, the van of the army is not attacked, so long as it does not remain in one place more than a few days, but the rear following in its footsteps is almost invariably attacked, whether it moves on simultaneously with the forces in front or not. Now, on the supposition that the drinking water has been the vehicle of contamination, it would seem highly improbable that it should always be changed from the wholesome to the noxious state, by purely natural processes, at these particular times. Again, at every gold-field of this colony of any size—no matter where (except in high mountain ranges, as in Gipps Land)—dysentery appeared, but never under a month or six weeks, or more, according to numbers, seasons, and local peculiarities. Wherever a rich tract of auriferous ground was opened up, there, sooner or later—the time being chiefly determined by the period of the year—an attack of endemic malignant dysentery would occur. If this had been brought about solely by the agency of drinking water, it should have followed that smaller and detached parties of miners procuring their water supply from the same creek, or chain of water holes, a mile or two above or below the larger encampment, should have experienced the same effects. But

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* This extract from Dr. Parkes is illustrative. "Davis" (on the Walcheren Fever) "mentions as a curious fact, in reference to the West Indies, that ships' crews, when ordered to Tortola, were 'invariably seized with fluxes,' which were caused by the water. But the inhabitants who used tank (*i e.*, rain) water were free; and so well known was this, that when any resident of Tortola was invited to dinner on board a man-of-war, it was no unusual thing for him to carry his own drinking water with him."—(Practical Hygiene, p. 52.)

this did not happen. When sailors fill their water casks at ports, they may or may not take "bad water" on board; but there is no instance on record of any ship's crew having become affected with dysentery from water taken in at an uninhabited, or a sparsely populated, country. None of the explorers in Australia ever met with dysentery among the aborigines, or, with one exception, suffered from the disease themselves. Sir Thomas Mitchell crossed the Murray and travelled through Australia Felix in 1835. From that time, the country was rapidly taken up and occupied, but dysentery was never heard of in the interior (excepting in isolated cases which admit of explanation), until the miners rushed thither in 1851 and 2. Yet if water could become polluted with its specific poison by changes effected without any interference on man's part, it would seem marvellous that these changes should always have been coincident with his appearance in large numbers. There is no occasion, however, to discuss the point further here. What may be now wanting to the completion of this part of the case will be evident enough, perhaps, hereafter. (c) In order to deal with the last question, water was assumed to be the only mode of communicating dysentery. But although this assumption was made, it is manifest that, although drinking water is undoubtedly a large and most potent source of the flux in some countries, yet that dysentery may, just as undoubtedly, attack populations in any part of the world in an endemic and epidemic form, without the drinking water being in the slightest degree connected with it. To exhibit this there is (1) the Dutch case at the Cape. There the troops had to remain at some distance from the water supply, and the water had to be conveyed to them from the time they took up the position, until long after the period of commencement of the endemic. In order therefore to connect the water with the dysentery, it would have to be shown that bad water requires a month for the development of its effects, or that this water became bad after the troops went to the sandy plain, and subsequently became right again when they moved nearer to it. But not to delay over these speculations, I will come to a much stronger case (2) with which I am better acquainted. The water supply of Melbourne was derived from the river Yarra until the Yan Yean reservoir furnished it in 1858. Malignant dysentery occurred in the city in 1853, 4, and 5. There was the same water supply for many years before the epidemic appeared, and for three years after it disappeared. Unless, therefore, it be assumed that the water cleared itself by some means, and that the conditions for the development of the specific poison did not come together again, it is difficult to conceive how the water supply of Melbourne was concerned in the epidemic. But the water supply to the gold-fields (3) presents a stronger case still, against the necessary connection between water and dysentery. The miner got his water for drinking and culinary purposes from rivers, creeks, natural or artificial water-holes, and from the shafts of claims, when they became quasi wells. And the miner gets his water supply, on every gold-field with two or three exceptions, from precisely similar sources to this day. Yet, notwithstanding that larger populations have occupied the surface, and that there has been a relative increase in decaying organic matter in the soil, to find its way by natural drainage to the water levels, epidemic dysentery has not made its appearance on any of the old established fields for the last fifteen years. This fact appears conclusive, whether it be regarded from the spontaneous development, or from the artificial production, point of view. If from the former, it is inexplicable how, and inconceivable that, by natural forces and processes only, a particular momentum should produce, or groups of momenta should combine to produce, in water, that specific infective substance which is capable of inducing dysentery, precisely at those junctures when large numbers of men settle in localities in any part of the interior, except the mountainous regions; that these momenta should not have combined in a similar manner, or should not have been known to result in the production of the dysentery germ in the water at the same spot, before; and that, after continuing in combination and activity for a period, variable, but limited to two years and a half, these momenta should then be broken up, or cease to concur, and should not meet, or be brought together, again, in the same local waters, for fifteen years. Indeed, combining this invariable result of the opening up of new gold-fields with what was observed before (b)—that detached parties of miners a short distance from the larger fields did not contract the disease—it is negatively, but overwhelmingly, evident that not only was the drinking water of the miners not converted into "bad," or dysenteric, water, by any spontaneity in, or accidental arrangement of, natural laws, but that, however the dysentery germs were introduced into the bodies of the masses of men then present, they could not have been conveyed in the drinking water.

25. (4.) The history of the disease is replete with instances in which the crews of ships have been free from dysentery until the end of the voyage, or until the vessel has touched at some intermediate port; when, without change in the drinking water, and without going on shore, the ship's company has been attacked with the affection. (5.) The disappearance of the malady in England—in London especially—was irrespective of alterations in the water supply. (6.) In the classical Gulstonian Lecture by Dr. Baly, so often quoted in modern references to the subject, the learned and elegant physician says:—"There are other influences from which dysentery might be supposed to arise, namely, diet, the water used as drink, defective ventilation, and defective sewerage. None of these, however, can have been the efficient cause of the disease in the Penitentiary. I think it unnecessary to detail on the present occasion the facts by which this has been rendered certain." We may assume that Dr. Baly took such measures as warranted the assertion to his fellows, that the "water

used as drink" could not have been "the efficient cause" of the disease; and most persons will be disposed to accept this authority as final. At the same time, I cannot but express a regret that this high authority should have considered himself absolved from entering into detail when addressing the College of Physicians. No doubt, to have done so might have proved somewhat wearisome and uninteresting to those present; but in the endeavour to avoid overloading his subject with unnecessary matter, and to steer clear of the prolixity of redundancy, he has deprived his Lecture of a large amount of scientific value. The experiments, measurements, observations, and facts, connected with the one subject of sewerage would have been highly interesting at the present moment. Exact information, for instance, touching the size, position, depth and fall of the drains, their state of repair and efficiency, the precise nature and probable amount of the fluid, or semi-fluid, material discharged, the periods when they were flushed, the ultimate destination of the matters flowing through them, and other details of a similar kind, would have proved of importance, to subsequent enquirers, though it might have palled slightly on his listeners. This admirable Lecture, indeed, is chiefly valuable from the accurate and vivid account it gives of the disease, its complications, and its pathology. As a contribution to the etiology of dysentery, however, I must be so bold as to say it is wanting in many essential respects. It fails to convey a faithful picture of the internal life at the Penitentiary in its sanitary aspects. There is nothing to show how the garbage and other refuse matters were disposed of—whether detained within the walls of the prison enclosure for any length of time, or got rid of periodically before they accumulated to any extent. One has no means of judging of the probable mass, or values, of putrefaction going on in Millbank at the time of the outbreak of the dysentery. There is no mention even of the mode in which the excreta of the prisoners were dealt with. Perhaps, the views connected with ventilation in Dr. Baly's day were not on so thoroughly sound a footing as subsequent investigation has placed them. Had he given the data to enable us to determine the cubic space allotted to each prisoner, it might be found that the ventilation was not quite in accordance with modern requirements. These, and other omissions, detract materially from the usefulness of the Gulstonian Lecture, not only as regards dysentery, but also with reference to its congeners—typhus fever and enteric fever.

26. But to return to the immediate question (c). It must be held that although dysentery germs can be formed, or can be preserved in an effective, or active, or potent, state, in water, and that although, therefore, water is a large and fertile means of infection; yet that they are quite capable of being engendered without immersion of the materies in water, and may be communicated readily to large numbers of persons in other ways than by the medium of water. Drinking-water, in short, may be perfectly pure, and yet those using it shall be affected with malignant dysentery.

27. On the last point, (d) my means of observation have been too limited to allow of my arriving at any satisfactory conclusions at present. I have neither had time nor opportunity to think the matter out. The precise conditions necessary to convert wholesome into bad water, are beyond me; and they must be left to others to determine. Whether the dysentery germ is perfected before it gets into the water, or whether it is capable of being developed after the basic material finds its way into it, or whether both these modes of water pollution may occur and coexist, I know not. But I can give future investigators the clue to the mystery, I believe. The original source of the dysenteric poison is the same, whether it be fabricated in water or out.

28. It is evident, I submit, that the endemic outbreak in the Dutch camp at the Cape, is not ascribable to atmospheric changes, or to the contamination of water. Malaria from the soil must also be set aside, for other reasons than that of the sandy plain. That by itself might not be conclusive to some minds, because it is stated that sandy soils and desert lands may be malarious from the nature of the subjacent strata. Taking the sandy plain, however, in conjunction with the other reasons for excluding alluvial decomposition from the causation of dysentery, it presents a strong case against that form of malaria. There must have been some other means, then, by which the Dutch force became affected.

29. While trying to bring out the cause of the disease, a difficulty presented itself very early in the enquiry. This was the explanation of the occurrence from time to time of sporadic, or idiopathic, or isolated, cases of dysentery, in the midst of populations among which the malady was unknown, or had not appeared for many decades. This spontaneous generation, or this fresh manifestation of a specific morbid poison, having only a very circumscribed local effect, tended to complicate matters, and to increase the obscurity. The dormancy of dysentery in England, and its occasional outburst, as alluded to by Dr. Aitkin and others, might depend either on the burial, or locking up, of the complete germs, which retained their vitality, or upon a reproduction of germs *de novo*. There was some analogy between dysentery and cholera in this respect, and there might also be a similarity between the modes of their germ development. Anyhow there were the sporadic cases to account for, and it was clear that any theory of causation which should not include these cases, could not be sound or complete.

30. Then again there was the very remarkable facility with which the poison of dysentery could be generated. Unlike some of the zymotic poisons, which apparently require elaborate or rare conditions for their evolution, the dysentery germ was clearly producible by extremely common factors placed under the simplest of conditions. The specific poison of yellow fever exacts such rare combinations of elements and forces for its spontaneous development, that many writers believe it cannot now originate in any part of the world, and that the only means by which it can possibly occur, where it has not previously appeared, is by direct transmis-

sion from an infected place. But here dysentery is the converse of yellow fever. For there is no country where its poison may not rapidly develope under ordinary, everyday, circumstances. Armies furnish the factors copiously and supply many of the conditions readily, whenever they halt for a given period, even though there had been no previous trace of the disease on the spot. And it might be safely predicated, as matters stand now, that any army actively engaged in warfare in any part of Europe, (Great Britain not excluded) would in three months of spring, summer, or autumnal weather—the first and last especially—breed endemic malignant dysentery. It actually occurred the other day, in the French and Prussian war, notwithstanding the utmost efforts of the medical staff. In fact, every army that ever took the field was a huge factory of dysentery. So again with the large congregations of men in search of gold. It happened in California and it happened here, and it would happen again to-morrow at a new "rush." It has occurred so frequently that it may be regarded as a regular accompaniment to a new gold-field. In fact, granting the local and seasonal favouring conditions, it is a constant. Then there are the endemic outbreaks in coolie ships and slavers. Men are taken on board in perfect health, and there has been no previous history of the disease. And yet after putting out to sea there shall occur the most fearful ravages from dysentery.

31. The ease and rapidity with which the dysentery germ may be developed and may produce its effects upon the human organism, would seem to argue a more simple and direct mode of action than that of the cholera poison. There is no occasion to assume the existence of a substratum, or nidus, for the ripening of the dysentery germ. The theory of the full development of the choleraic specific poison, according to Pettenkofer, involves a cholera germ emanating from a centre (Hindostan). This cholera germ travelling into Europe by various modes, is incapable, of itself, of producing cholera. It must first meet with its substratum (in the soil) before the necessary conditions can be found for its due perfection or conversion into the ripe cholera poison. Whether this solution of the cholera problem be correct, or not, is not now the question. It is introduced to show the striking contrast in the (hypothetical) causation of the two diseases. For whatever theory may be finally adopted as regards the cause of dysentery, it is quite evident that it will neither require the existence of one central source, nor the mediation of a second hatching-place for its germ.

32. There is another peculiarity in connection with the dysentery germ, though it is not an absolutely distinctive characteristic. An attack of dysentery does not lessen the susceptivity to dysentery. It does not confer an immunity on the body, or leave it in a better position to resist future attacks of the same disease; but, on the contrary, it seems to render it more prone than before to succumb to subsequent invasions. There is no acclimatising the system against this fell malady; for the longer the exposure to the cause, the more certain, rapid, and fatal the result.

33. Whenever dysentery occurs in the epidemic form, it is always accompanied by either typhus, or enteric fever, and not uncommonly by both. This intimate association of these three diseases would seem to indicate a certain, and not very remote, relationship, if not a close consanguinity, in origin. The "low organisms" which are supposed to be concerned in the causation of the two fevers may have a similar source to the dysentery germ, if not an identical one, with some slight modifications as to conditions.

34. There are other special matters that might be referred to connected with the specific poison of dysentery; but enough has been said, probably, to show the reader that in offering the within simple looking theory for consideration, it has not been hastily adopted. He will also find that the very difficulty of discovering a cause sufficient to account for all the phases of this remarkable disease, was converted into an actual facility for recognising its fullness and adequacy, when the idea was once struck. Before detailing the propositions and offering further observations in support of them, I have yet a few remarks to make.

35. When I had satisfied myself that the leading and essential principles of the views which had occurred to me were substantially correct, however defectively worked out and wanting in completeness of detail, the magnitude of the consequences of those views was apparent. Several grave doubts then suggested themselves as to the possibility of there being some fatal error somewhere, which would step in to the overthrow of the conclusions when laid hold of and brought forward by some acute reasoner. The doubts referred to resolved themselves into the following shape:—(*a.*) Is the dysentery of New Holland identical with that of other countries? (*b.*) Is dysentery the undoubted product of a morbid poison? (*c.*) If so, is one sole, special, peculiar, distinctive, or "*specific*," poison, only, concerned in, and capable of, producing the disease? (*d.*) And, in that case, can this specific poison be developed by more than one factor, or one set of factors? It was obviously of primary importance to get at something clear and decisive on these points, for the whole thing turned on them. And yet an investigation of this kind was rather beyond the limited range of a mere amateur, or dabbler. I was much in the position of a poor "prospector." I had struck a promising vein of thought, and had not the mental capital to work it. However, with the kindly help of one or two medical friends, who put me into the proper groove and lent me some tools, I have made out what follows. It may serve the purpose.

36. (*a.*) Upon this point there is no room for doubt. All the medical authorities here, have been unanimous in their accounts of the manifestations of the disease, of its anatomical lesions, and of its pathological appearances. On referring to the Australian Medical Journal for 1859, I found (p. 174) an account of a discussion in the Medical Society on a paper read by Mr. Ford, Surgeon to the Police Force of Victoria—"On the Treatment of Dysentery."

In that paper Mr. Ford refers to the local effects of the disease in the large intestine, and also stated that "he had been very careful "to quote only such cases as were undoubted dysentery. During "the period, many hundred cases of severe diarrhœa, approaching "to dysentery, had been treated." In the remarks which followed, Dr. Cutts (one of the Physicians to the Melbourne Hospital) speaks of the "engorgement of the mucous membrane of the large intestine, and subsequent ulceration and sloughing." Dr. Tracy (Lecturer on Midwifery at the Melbourne University) and Dr. Barker (Lecturer on Surgery) followed on the subject of treatment. Mr. Thomson observed that "no one could have much doubt as to the seat and nature of dysentery who had had any opportunities of seeing it, or who had looked into the works of Ainsley or Carswell." [Mr. Thompson, I found, by the bye, had forestalled me in one particular]. Dr. Robertson (Lecturer on Medicine) said that "in a case of chronic dysentery where he had an opportunity of making a post-mortem examination, he found the morbid appearances confined to the colon," &c. It is not to be supposed that men of this stamp did not know what they were talking about. But to place the matter beyond the reach of objection, there will be found, at the end of the work, some extracts from evidence given at the Yarra Bend Lunatic Asylum. I think these should carry conviction to any mind that the disease as it has appeared in this colony is veritable dysentery.

In addition, I may mention that I have spoken with medical men who have been familiar with dysentery as it appears in the East, in South America, in California, and in many parts of Europe; and they have all stated that there cannot be the slightest doubt that the dysentery of this country is the same in all respects, and that there can be no possibility of mistaking its characteristic symptoms and its invariable results as regards the glands of the large intestine. Therefore, with this concurrence of testimony, the point must be taken to be settled.

*37. (*b.*) All writers agree that certain classes of diseases are produced by some poisonous agent, or by some agent that produces morbid effects upon the living body. And they all include dysentery among the classes of diseases which are produced in this way.

38. (*c.*) As regards this issue, the law seems to be that when a disease of this kind invariably presents some prominent features, some well-marked, characteristic, diagnostic signs, by which it may be known and distinguished from all other diseases, it has been caused by some one special, peculiar, distinctive agent, which is called a *specific poison.* As corollaries:—Each specific poison causes its specific disease: and no specific disease can be caused by other than its specific poison. Thus small-pox can be caused only by its specific virus; cholera by its specific infective matter; and mumps by its specific agent. Dysentery has such unvarying features, and is accompanied by such certain and infallible lesions of one part of the body, that it is universally recognised by every

writer of modern times as a disease having its origin in a specific poison. It is now not questioned that the dysentery which ravaged the allied armies before Sebastopol, was the same disease as that which followed Moore to Corunna; or that the latter affection was identical with the dysentery at Flushing, where it was so deadly that it was brought specially under the notice of Napoleon, and elicited the remark—"*L'homme meurt partout.*" Then again the dysentery of Marlborough's day was precisely that alluded to by old Froissart, in his account of the forces under the Duke of Lancaster, in Spain, when exposure, &c., "flung them "into fevers and fluxes so as to carry them off instantly to their "graves: * * * for these disorders spared none." And indeed the bloody flux may be traced to the remotest ages.

39. If, then, dysentery is the same now as it ever was, it follows that it is caused now as it ever was. If it be caused by a specific poison, the same specific poison must have caused it through all time. It has been shown that the flux of New Holland is identical with that of other countries. Therefore, the epidemic malignant dysentery which attacked the diggers on the Victorian gold-fields was caused by the same specific poison as that which occasionally gives rise to an idiopathic, or isolated, case in London, and periodically sweeps off its thousands on the European continent. By the fatal sign on which Hippocrates laid his finger, we know that the same specific poison caused the same specific disease among the Greeks of old, that has never been absent from Hindostan since it was peopled by the present races, and that will infallibly break out at the next new mining "rush"—provided only the local and seasonal conditions be there.

40. The law of specific poisons being granted—and it is at least as fixed as that of gravitation—no possible combination of circumstances can bring about a single case of dysentery, but that which produces its specific poison. No number of predisposing causes, and no amount of susceptivity, or receptivity, can result in this specific flux, if its specific poison be absent. Any theory of causation, therefore, which does not provide a cause for every case of dysentery that ever existed, must, of necessity, be unsound. An efficient cause in one instance must be shown to be an efficient cause in all other instances. At least that is the view I take of a specific poison, and a specific disease caused by it. And the inferences I draw seem to me so direct that I cannot understand how they should be ignored, as they are occasionally by moderns. They are so plain, so easily deducible from the premisses, that I should have hesitated about setting down such platitudes, but that they would seem to have been lost sight of by some of those enquirers who have set about investigating the causes of diseases having their origin in specific poisons. Even at the risk of being didactic, I have thought it advisable to arrive at some definite understanding upon this point. For there is such a seeming confusion of ideas in some of the observations of writers touching this particular matter, that it is as well to see how it really stands. I observe

certain curious things in currency and passing easily as good coin, and it seems high time to throw them down on the hard counter of reason, and try their ring. If the law relative to specific poisons be not as I have put it—if that law has been superseded and some larger natural law substituted—I am all wrong. If, however, that law stands as yet, as I suspect it does, it puzzles me somewhat to find such palpably heterodox doctrine afloat. At present I can only conclude that one sole, special, peculiar, distinctive, or specific, poison is the cause of dysentery.

41. (*d.*) Whether more than one factor, or one set of factors, are capable of producing a specific poison, is one of those questions which may appear to Europeans purely speculative, or to have little or no practical value or interest. Like that as to the spontaneous origin of rabies, or as to the possibility of the independent production of the virus of small-pox, it may perhaps be regarded as somewhat idle and useless. I think I observe a tendency to throw a little learned cold water on those who have taken some pains to ascertain whether the specific poison of yellow fever can be developed in a country fitted apparently for its development, but in which it has not yet occurred. These philosophical enquiries would seem to be looked upon in some quarters as having about the same relative value to medicine, as the ingenious disquisitions of theologians on the number of angels that can dance on the point of a needle, had to religion. But with all deference to the really great men who seem to have conceived some such view, I submit there is a lack of cosmopolitanism in declining to enter upon the investigation of this class of subjects themselves, and in looking passively, if not somewhat contemptuously, upon those who have turned their attention to the study of these things. Because a question has no direct bearing and no immediate reference to their own country, is not a sufficient reason for throwing it aside as worthless, even on the narrow ground of self-interest. For there is no saying what light might not be reflected on the causation of the zymotic or epidemic diseases of every region, by the actual discovery of the specific poison of a cognate disease in any region, however remote.

42. Whether it be due to lukewarmness, however, or to some other cause, must be left; but there is no recent exposition of the laws relative to the factors and conditions for the evolution of specific poisons, that I can find. There is nothing at all events by which to determine this important practical question. As it must be dealt with somehow, I will put together such views as I have gathered in as compact a form as possible—though I do not profess to reflect the latest views, or indeed any particular views but my own crude ones. The hurried glance I have been able to throw over this part of the subject, must be the excuse for any shortcomings. Such is the vagueness and cloudiness of its present position, that I hardly know whether I am lagging much behind, or shooting far ahead, of the philosophy of the day. My notions, however, may be taken for what they are worth.

43. It is assumed to be proven that dysentery is caused by a specific poison. The question is whether that poison can be produced in more ways than one. Practically it may be possible to show that it can be; but it must be impossible to demonstrate the proposition that it cannot be. This will be evident at a glance, if the venue be changed to hydrophobia. It cannot be affirmed with certainty that the only source of the hydrophobic poison is in the canine race, because as yet no other known source exists; for it cannot be gainsaid that the factors for the specific virus may possibly exist in some other class, or classes, of animals, for all we know, though the conditions may never have met. It is evident, therefore, that there may be other modes of producing the specific poison of dysentery than the one, even if we fail in discovering them.

44. But if we accept reasoning by analogy as a tolerably safe guide in questions of this kind, we may fairly assume that the natural laws by which the development of specific poisons is effected, are as fixed and unalterable as all other natural laws. Although it may be impossible to prove that the power of producing the hydrophobic virus resides only in the canines, yet the chances against any other family having the power of producing even a similar virus, must be many millions to one. Considering the thousands of years during which any such power has been latent, or not known to man, the chances against its inherent existence must be enormous. But this is merely as regards a similar virus producing similar effects to hydrophobia. If we attempt to calculate the probabilities as to any other family of quadrupeds having the power of producing a virus, identical in composition and identical in its effects on the human system, with that of rabies, we should soon be lost in the numerals we should have to handle. For if we take one little division of such a vast calculation, we shall soon be satisfied as to the immensity of the whole. This large island of New Holland has been occupied by the British now nearly an age—the first batch of convicts landed in Botany Bay in 1787—and during the whole of the period not a single instance of rabies has been heard of, either among the native dingoes, or among the imported dogs. Nor have the blacks any traditions tending to show that the dingo has ever been known to go mad. And yet the conditions popularly supposed to be required for the development of rabies are constantly to be found here, viz., heat and scarcity of water. If, then, in a hundred years the factors and momenta for the development of the virus have not met in the canine race in this country, by how many are the chances increased against an identical virus to that of the canine races elsewhere being developed in any other race? In fact, it may safely be accepted as a sound natural law that the hydrophobic poison can be produced only in one way.

45. If we turn to any of the so-called zymotic diseases having a closer analogy to dysentery than the peculiar affection resulting from the introduction of the virus of a mad dog, we shall find the

same unity in causation. There is only one known cause of small-pox, and its origin is unknown. How the factors and momenta, whatever they may be, first came together to produce the specific poison of this disease, has been discussed by the world, but is generally dismissed as unprofitable, because the sole way in which the malady now occurs is by contagion. Some authors go so far as to say that small-pox cannot be engendered spontaneously, and that, if it were possible to stamp the disease out of the world, it could never appear again. This, however, rather jars upon common sense, as it seems to me, but I refer to it as showing how firm is the belief in the fixity of the laws of the development of a specific poison. For here, it will be observed, it is not a question whether the specific infective matter of small-pox can be developed in more ways than one. That question seems to be utterly unworthy of a moment's consideration. Not only have enquirers settled that there can have been but one source from which this poison came, but that even that one source is dried up for ever. There was but one mould and it has been broken. Nature has lost the secret of reproducing small-pox, as men have the art of staining glass.

46. All this is clearly unsound and vicious. To say that the chances are so many for, or against, a particular result from a recurrence of a particular combination of circumstances, is one thing. But to say, either that the result would not follow if the same combination were brought about, or that a combination which has once occurred in obedience to natural laws, can never occur again in obedience to those natural laws, is quite another thing. I can understand that the spontaneous development of the specific virus of small-pox may be a very remote contingency—though it might take place somewhere to-morrow, or may have taken place somewhere yesterday—but I cannot take in the proposition that, because a product which has resulted once from unknown elements and forces, has not been formed a second time within the knowledge of man, it can therefore never be formed again. For if we examine the thing closely it appears to come to this. The virus was formed somehow, somewhere, at some time. That it must have been formed by some rare combination is evident enough. Assuming that the combination took place on a certain day, it follows that the world was free from small-pox before that day. Were not the chances against small-pox occurring apparently greater the day before it actually occurred, than they are to-day against its (spontaneous) occurrence anywhere to-morrow? And yet it did occur. However, I must not allow myself to be shunted too far upon this siding. The main line of argument is that small-pox cannot have more than one cause; and if it be successfully maintained that the original cause cannot be reproduced, and that the only possible way in which the disease can now be propagated is by contagion, *a fortiori*, any other causes are out of the question. It will be admitted then that small-pox is caused by one specific poison, and that that specific poison is gene-

rated in only one way. The same is said of measles and scarlet fever. Their origin is lost in obscurity—they are handed down from generation to generation by contagion solely—it is supposed.

47. Yellow fever is another contagious disease on nearly the same footing with the three foregoing. The strong presumption that it has only one cause is warranted by the manner in which it has spread. Countries having precisely similar conditions as to latitude, soil, &c., as those in which yellow fever has prevailed, have remained exempt until the specific poison has been conveyed to them and has become naturalised. That the conditions for the evolution of this poison are very rarely brought together, may be deduced from the fact that writers hold about the same views as to its spontaneous generation that are held with regard to small-pox. I shall have more to say, however, as to the probable origin of yellow-fever, which does not appear to me so obscure as that of small-pox.

48. Very little is known as to diphtheria, but that little points to its having but one specific poison evolved by the same factors through all time. The appearance, disappearance, and reappearance of this obscure malady during past ages, with long intervals between the epidemics and the total disappearance of sporadic cases for considerable periods, not only argues one specific poison built up by one set of factors, but would seem to show that the disease has had an independent origin, when it has broken out after leaving the world for many years. One of two things must have happened. Either the specific poison of diphtheria has been stored up between the outbreaks in some mysterious way, as the choleraic poison is in Europe occasionally, or the factors have been brought together under the same conditions as before, with the same result. Of the two views, I am more disposed, at present, to take the spontaneous evolution theory. The point, however, at present, is whether the specific poison itself can be evolved by more than one process; and upon that point, I submit, there can be but very little doubt.

49. The several specific poisons of leprosy, glanders, hooping-cough, syphilis, intermittent and remittent fevers, cholera, and many other diseases, seem to be all formed by their special factors. Very little is at present established in connection with their origin, however, in any instance except one. Indeed at this moment the world would seem to be in perfect darkness as to the conditions of evolution of more than a single specific poison. Enough is known of some of them, though, to establish the law that they are to be produced only by the arrangement of certain elements in a special manner. And the same law may probably, if not undoubtedly, be applied to all of them.

50. There are two kinds of fever, having such strong marks of resemblance and occurring so frequently under such similar external circumstances, that up to a recent period they were considered to be merely modified forms of one disease. It is only within the last few years that typhus fever and enteric (*olim* typhoid) fever have

been pronounced to be separate and distinct affections, each caused by its specific poison. This division has been effected by means of a series of careful and shrewd observations, aided by sound inductive reasoning. Each disease has its peculiar eruption and its own local internal lesions, which are now readily recognised by the physician. As these two fevers are intimately associated, as to origin, with dysentery, and as there are fair grounds for believing that what will throw light on any one of the three diseases will tend to clear up the causation of the other two, I will postpone enquiry as to the factors concerned in producing these specific poisons until after dealing with that of dysentery. It may be said here, however, briefly, that the whole evidence goes to show that these specific poisons are formed in only one way.

51. The animal and vegetable kingdoms furnish innumerable instances of specific products from the same factors. In fact, there is but one law of unbending uniformity. The same causes must produce the same effects, and the same effects can only be produced by the same causes. By way of sharp illustration there are the different venoms of snakes, which are formed in all countries from the same classes of materials and apparently in the same manner. The fluid containing the poison has the same chemical composition in all the instances where an analysis has been made. Organic chemistry has failed to detect any variation in the elements, or their quantities. The formula for one venom stands as a formula for all venoms. And the miscoscope does not reveal any difference in the arrangement of the molecules. There is no appreciable distinction between one venom and another, therefore, and yet each venom has its own special, peculiar or specific, effect on the animal organism. The bite of the rattle-snake, of the cobra, of the coral snake, and of the deaf adder, is followed by its own train of symptoms and effects, each well-marked and clearly defined. If it be supposed that a difference in nutrition may to a certain extent assist in accounting for the difference in the venom of snakes found in countries widely apart, there is the fact that in some places not far from Melbourne one may procure, from the same swamp on the same day three kinds of snakes, each of which, though subject apparently to precisely similar conditions, shall have a perfectly distinctive, or specific, venom.

52. No attempts to change the specific products of vegetation have ever succeeded, or can ever succeed. The correlation of vital forces is such that one eternal law fashions all results. Cultivation may produce a variety, but cannot create a species. And by the fruit, the tree may always be known. There is no occasion to point this by illustration, for it is too patent to admit of doubt or question. I will pass at once, therefore, to the conclusions I have formed—whether rightly or wrongly I must leave—on the subject of the present question (*d*).

53. It appears to me that the conditions for the production of a given specific poison must always be the same. And every specific poison has its specific conditions of evolution. In other words,

it is required for the formation of a given specific poison that it shall always be formed out of the same elements brought together under the same circumstances, in all essential respects. Unless the exact ingredients required, and the particular conditions required, are brought together in the precise way required, the product will not be the specific poison required. But if factors or conditions that have once resulted in a specific poison being formed, be again brought together, at any distance of time or place, the same specific poison will be reproduced. This is a constant. It follows conversely that a specific poison which has once been produced by a particular combination of elements and forces, cannot be reproduced until, or unless, the same combination occurs. No other arrangement of particles and no substitution of processes, can effect the same result. Any alteration of the materials, or any disturbance of the conditions, interferes with the specific product. So it may be determined that, when a given specific poison has not been developed, where its development is looked for, the factors and conditions have not been present; and that when a given specific poison has been developed contrary to expectation, its conditions must necessarily have met.

54. To sum up this enquiry entered into for the purpose of resolving doubts:—If the chain of inference be not weaker than it seems to me, it will amply support the propositions that (*a*) the dysentery of New Holland is identical with that of other countries; that (*b*) dysentery is the product of a morbid poison; that (*c*) the specific poison only is capable of producing dysentery; and that (*d*) the specific poison of dysentery cannot be produced by more than one factor, or one set of factors.

A NEW THEORY OF THE CAUSE OF DYSENTERY.

55. I submit that there is but one cause of dysentery, and that that cause is human excrement on the surface of the soil. And, further, as a corollary, that human excrement is incapable of causing dysentery, when undergoing any kind of change or decomposition in the soil. The view I take, plainly and simply stated, is that this disease occurs when fœcal matter is left on the surface of the ground at seasons favourable to certain changes in this substance; that these changes result in the evolution of certain products which are given off and pollute the atmosphere—causing the specific effects of dysentery; and that as a consequence, sporadic, endemic, or epidemic, dysentery, cannot occur, unless fœcal matter be exposed as indicated. Moreover, the noxious emanations from excrement which cause dysentery cannot be given off below a certain temperature; nor can they come from excrement at any temperature without moisture; nor when excrement is covered with earth; nor when it is contained in any kind of receptacle placed below the surface of the soil for the purpose of receiving the excreta as they come from the body. [The question of water-pollution is excluded from these views, and will be dealt with apart.]

56. And this is the whole pith of the thing—the sum and substance. The reader will not be surprised perhaps that I was nearly dismissing so simple and, apparently, so self-evident a cause of dysentery from my speculations, on the ground that it must have been examined into before and found insufficient. He may, moreover, refuse to credit the possibility of this explanation of so terrible a malady being the correct one. He may be disposed to treat it with contempt. Or he may even find in it subject for ridicule. It has its comic side no doubt for some minds. Yet the flippant might be startled if they had the capacity to apprehend the consequences of this view of the cause of dysentery—if correct. It might arrest the coarse jest of the common soldier even, to be told that of all the thousands of his comrades who died in the Crimea in the horrible tortures of this, the most dreaded and most fatal of all camp diseases, not one need have sickened had this been known and had the Provost-Marshal done his duty. For it will be pateut to the reflective, that should this simple notion turn out to be right, the discovery of the cause of dysentery leads straight to a ready practical means for its prevention. No great inventive genius is required to devise an efficient mode of application. In the attack on Sebastopol, for instance, it was but to sink a few shafts in the saps and parallels sufficiently deep to prevent

their overflowing from the rains; or, if this was impracticable, from the nature of the strata, or from any other cause, to have introduced some form of handy portable latrine. At the rear of the tents a few rows of ships' water-tanks would probably have answered the purpose. Even if they required emptying, or abandoning, and the substitution of others, it would not have cost much. The French, of course, with their turn for expedients, would have kept their quarters free from danger; but if our other allies, the Turks, trusting entirely to Allah, refused to conform to so serious an innovation, and persisted in spreading pestiferous germs throughout the neighbourhood, either their assistance at that spot might have been declined, or we should have undertaken their hygienic work for them. It would have cost less in blood and treasure. As for the camp followers—the storekeepers, the grog-vendors, and the whole tribe of hangers-on—there need have been no great difficulty with them under martial law. A proclamation hinting at whipping, deportation, and other summary modes of dealing with offences, coupled with a vigorous administration of the prescribed regulations, would have had the effect of preserving the air from noisome and deadly effluvia. But all this is premature. Much has to be done yet before its truth can be affirmed. Still it may serve to bring one of the facets of the subject into view and thus relieve it from the grossness of its aspect to the educated Englishman He will disregard the nauseousness of details while looking at the vast issues depending:—issues not only affecting the well-being of armies, but the health and life of the whole civilised and uncivilised world. It is true his own country is fortunately free from this plague of dysentery, because England spontaneously and unwittingly adopted the only national means of stamping out the foul disease; and no purely British community in any portion of the globe will ever be liable to epidemic dysentery when it has the means of adhering to its inherited or acquired habits of decency and cleanliness. But he must not forget that all other nations besides—except a few populations—have not adopted the salutary customs that his ancestors took up some hundred and fifty years ago; and that, consequently, they are still subject to the visitations of fearful epidemics of dysentery. He has only to look back to the authors quoted to realise what dysentery in a country really means and involves. And, whatever present contempt he may have for the theory propounded as to the cause and prevention of so much disease, he will at all events see that the particulars I shall have to enter into are redeemed by the object in view. He may be reminded also that although England has undoubtedly got rid of dysentery, there are fair grounds for assuming that she has substituted other and equally fatal diseases, by the very means by which she happened to extirpate this one.

57. Although I cannot adhere rigidly to method in what follows, and must be held excused for not observing all the forms, I will lay down such propositions as will enable me to group the principal

arguments. The reader will find such evident marks of haste throughout this little volume, however, that he need not wonder at, and will perhaps forgive, any defects in the arrangement. In offering the following propositions, it will be understood that the mode of empoisonment of water is left out of consideration for the present. That source of dysentery being excluded, I submit:—

I. That the elements essential to the formation and development of the specific poison of dysentery are contained in human excreta.

II. That a certain degree of temperature; a certain amount of exposure to the influence of the atmosphere; and a certain quantity of water, or moisture in the air; are, each and all, necessary conditions for the evolution of the poison from fœcal matter.

III. That the quantities, or values, of the poison formed, are determined by the amount of excrement submitted to the conditions; and by the more or less perfect application of, or submission to, the conditions.

IV. That the poison thus formed passes into the atmosphere, and is capable of producing dysentery in those exposed, to a certain extent, to its influence.

58. It must be admitted that the second of these propositions is somewhat vague, indefinite and bald. Yet to those who apprehend the difficulty of laying down general principles upon insufficient data, it will be evident that I have no adequate means, at present, of fixing or limiting the range of temperature within which the dysentery germ may be formed; or of determining the period of exposure necessary either to complete its formation, or to destroy the power of forming it; or of saying what precise amount of water is absolutely required for, or what degree of fluidity might interfere with, the perfecting of the poison to be disseminated through the atmosphere. All these points and many others which will be alluded to, as well as the vast number of important considerations which have been left untouched, must be relegated to future explorers in the field. I have been unable to do more with the imperfect materials at my disposal. I have not been in a position to make an experiment, or to utilise the microscope. I have nothing, in fact, to offer in support of the theoretical views advanced as to the causation of dysentery, but what has come from books and reflection. Therefore I have not hazarded in these propositions more than I shall be able to substantiate, as I believe, by argument.

PROPOSITION I.

59. It will be sufficient to observe here that the source of dysentery pointed out fulfils one of the great essential requirements in all explanations of causation. It is one of the very few things that can possibly be concerned in this ancient and widespread disease. It answers to the test of universality. It cannot be rejected for want of catholicity. The other omnipresent and

ubiquitous causes which have been mooted—*e.g.*, air, water, earth, food, &c.—clearly do not furnish an efficient cause, either singly, or in any combination, for every case of dysentery that has ever been caused. They all break down somewhere. Though they may suffice, in some ways, to account for many outbreaks of dysentery, they fail here and there to explain the evolution of its specific poison; as, I venture to think, has been shown. If the dysentery germ, then, has been once formed when these agents have been absent, it follows that it could never have been once formed as a consequence of their presence. For, as has been seen, the factors and conditions of a specific poison are as specific as the poison itself.

60. Although non constat that the exhaustive process is final and conclusive in investigations of this nature—for the reason that it is impossible to know when exhaustion is exhausted—still, if it be shown that all other causes of dysentery are inefficient, it adds material strength to the presumption that human excreta are the efficient cause. If this assumed cause does not fail, where all other assumed causes fail, it is a good negative proof that it has been rightly assumed. But not only will it have to be shown that this theory still holds together where all other theories give way, but that it is not to be broken by any means. Whether the elements found in fœcal matter form the basis of the dysentery germ is to be tried by all the tests to which all other hitherto alleged causes have been submitted, and by all those which can be suggested. If it cannot do this—if a single instance of dysentery occurring in any age or country can be shown to have been brought about by other means—there is at once an end to it. If it be proved, either that some other agent has caused an attack of dysentery, or that fœcal matter could not possibly have caused it, the theory is unsound.

61. But if, on the other hand, the evidence to be produced be trustworthy and admissible, and will bear the interpretation put upon it, it will be seen that there are at least tolerably fair grounds for believing that the human excreta play a very important part in the production of the dysentery germ—if not the actual part here assigned to them. Perhaps nothing short of the absolute discovery of the germ itself within the body and the tracing it clearly to its source in the excreta, will be held to be positive proof of the correctness of the theory as to its origin. Whether this proof will be forthcoming will depend upon the microscopists perhaps;—that is, supposing the germ to be an organic substance—a tangible body. Yet without this visible proof, I submit that it may be quite possible to arrive at a safe, sound, logical conclusion that the active agent in the causation of dysentery comes from the source indicated. Even if the microscope, or organic chemistry, fail to bring out the precise nature of the agent, whatever it may be, it does not follow that it cannot be connected with one set of factors so closely, as to amount to a certainty that it is the product of those factors. Much more complicated problems have been

worked out by pure induction and have been subsequently proved to have been solved correctly. Not only astronomy but every branch of science and art can furnish instances of the kind. And medicine has at least one glorious instance of sound induction. Therefore, even in the event of the physical proof failing to substantiate the conclusions here arrived at, there is still the inferential proof left. Of course if microscopical or chemical investigations show that the dysentery germ cannot be evolved from excreta, or that it is the product of other factors, that is another thing. This theory then falls to the ground. But the merely negative fact that the germ, or specific poison, cannot be found; or that if found it cannot be traced to, or connected with, fœcal matter, is not sufficient of itself to invalidate, or upset, the theory that it has its origin in fœcal matter. It might tend to delay the acceptance of the view, but it could have no other disturbing influence, if the view be shown to be correct in all other respects.

But before going any further in this direction, it will perhaps be advisable to consider the first and second propositions conjointly. Together they form the keystone of the arch, and it will be more convenient to deal with them thus than separated.

PROPOSITIONS I AND II.

To avoid needless repetition it will be assumed that fœcal matter is the basis of the dysentery germ, or that it contains the elements essential to the formation of the specific poison. The mode or modes by which the elements are subjected to changes and rearrangements ending in the production of the active agent of dysentery have now to be considered. For convenience of reference, the subject may be divided into—A. *The dysentery germ.* B. *Temperature.* C. *Atmosphere.* D. *Moisture.* E. *Other conditions.*

[A.] The Dysentery Germ.

62. This great difficulty may as well be grappled with at once. It is the stumbling-block of causation in every direction—the one obstacle to the prevention of a large mass of disease. The subject is a wide one, and I shall have to travel out of the direct road to dysentery. Still, although the course taken may seem erratic and unnecessarily tortuous, the reader will probably find a "blazed" line of thought leading him eventually to the object he had in view when starting.

63. At the present moment the dysentery germ is hypothetical; unless the claim of Hallier, to be alluded to presently, be admitted as a discovery of the germ. It stands on nearly the same footing with the "typhus germ," and the "cholera germ," and the specific germs generally—except one. These germs have been assumed hitherto; though the German school of miscoscopists has shown that certain minute fungoid growths are present in the bodies of those affected with so-called zymotic diseases. These cryptogamic fungi have such well-marked and distinctive characters, that, it is said,

their cells, sporules and filaments, may invariably be recognised and determined and their species made out. The fungus of each of these diseases is, in fact, a specific fungus; and, according to Hallier, it is the active agent, or principle, or the specific germ, of the disease. Both Klob and Hallier have investigated the subject of the cholera germ very minutely. I have the work of Professor Klob now before me, but I regret that my knowledge of the labours of Hallier is very fragmentary, from having been picked up here and there in periodicals, or filtered through the works of others. Klob would appear to conclude, from the fact of its constant presence in the dejections and vomited matters of cholera patients, that a Bacterian fungus—the Zooglœa Termo—is the cholera germ. Whether this cryptogam is identical with the one described by Hallier as the cholera germ, I do not know; or whether, supposing these observers agree, the Zooglœa Termo is the true specific germ of cholera. Indeed I need not stop to enquire whether this microscopic organism is the active agent in producing cholera; for I have neither data, nor the necessary qualifications to enable me to do anything definite with them, if I had. I can only say that while the observations of these accomplished microscopists, and some others, appear to have been conducted with great care and to be deserving of the highest consideration, they are as yet but stepping-stones to practical results. Until these errant fungi which have found another substratum in the human system, are traced back to their nidus, or their parent source, their discovery in the body lacks its chief value. To connect them with their origin will be "the crowning of the edifice."

64. The only instance of the actual discovery, in its complete sense, of a specific germ, is that made by Dr. Salisbury, an American physician. As this vastly important discovery has by some unaccountable means dropped out of notice, and is not even alluded to in recent works, and inasmuch as it bears obliquely on this question of the dysentery germ, I must give a short account of it here. But first I will briefly refer to another cognate enquiry in which Dr. Salisbury was engaged, as it illustrates the remarks before made with regard to the attitude assumed, by some European savans, towards those who have gone into the matter of the origin of diseases which are supposed unlikely to occur *de novo*. Investigations as to the factors of the exanthems, syphilis, yellow fever, and the like, are held to be idle, speculative, and useless; for the simple reason that, as we have the diseases now, it is a waste of time to enquire how they originated—a most unsatisfactory and unphilosophical conclusion.

65. In the year 1862 Dr. Salisbury published in the American Journal of Medical Sciences his account of the curious connection between measles and mouldy straw. He states that the disease may be produced by inoculation and other infection with straw fungi. Is this the case? If so, does the scientific world think it valueless? From the fact that there is nothing whatever relative to the subject in later treatises on measles, I assume that Dr.

Salisbury's statements either want confirmation, or have been disproved. If the latter, there is no more to be said—but I have not fallen in with the disproof. If the former, it seems strange that so singular a set of statements should have remained so long without being thoroughly tested and sifted and dealt with one way or another. Hallier gives a cryptogam—one of the *Mucorini* —as the specific fungus of measles. Has this *mucor* ever been connected with mildewed straw? Or has the whole matter been allowed to fall into oblivion, because measles is established in the world and its cause is of no consequence? Yet who can tell but that the light reflected from a full knowledge of the origin of measles, might not be thrown on diphtheria and lead eventually to the elucidation of the causation of that malady?

66. In the same Journal (1866) are the results of some experiments made by Dr. Salisbury, with a view to ascertain the causes of an intermittent fever in the valleys of the Ohio and Mississippi in 1862. Substantially they are as follows. He procured the secretions from the mouth and air-passages of those affected with the ague, and found that they invariably presented under the microscope certain peculiar, distinctive, oblong, cellular, bodies, which he describes, very like palmellæ. There were many other cryptogams and cells and sporules of other species of fungi, but they were not constant. He set on foot some ingenious experiments in order to trace the connection between these cellular bodies and the palmelloid plants growing in the vicinity of the ague districts, and to determine the relation between them and the local miasmatic disease. Dr. Salisbury brought out that these peculiar oblong cells came from the palmelloid plants; that in certain stages and conditions they were taken up into the atmosphere by the night fogs or mists; that, with minute organisms of other kinds, vegetable and animal, they remain suspended in the damp air below a certain elevation; and that they caused the intermittent fever. He calls this *Palmella* the "ague plant," and he established its right to the title by curious and seemingly convincing proof. He took some earth containing the cryptogams, in a fit state to induce intermittent, to some hills about five miles away from all malaria. He placed this earth in boxes on the window-sill of a room, in which two young men slept, on the second story. The window was kept open, and both these martyrs got decided tertian ague, one on the 12th, the other on the 14th, day. Dr. Salisbury made yet another similar trial—equally satisfactory.

67. Assuming that due care was taken in these experimental essays to avoid all sources of fallacy, as well by the experimenter as by those experimented on; and assuming moreover that the statements have not been impugned—and I have no reason to believe they have by actual experiment—it would certainly seem that the ague in these cases was *propter hoc.* If so, this discovery is unique. There is nothing like it in the history of specific poisons. And Dr. Salisbury may fairly claim to have been the first to discover a germ and to connect it with its source. Other ex-

plorers have found cryptogamic fungi in zymotic diseases, but none, that I can find, have sheeted one home, as Dr. Salisbury has done, to its original substratum.

68. It is the want of this finishing stroke which leaves the labours of Klob, Hallier, and other microscopists in this position: —either the germs they have discovered may not be the specific germs which cause the diseases; or, conceding that they are, the inability to follow these fungi up to their hatching grounds deprives the discovery of immediate practical value. For, important as these partial discoveries must be eventually, still so long as the cryptogams which find their substrata in the human body and cause disease there, cannot be connected with the particular substances from which they spring, it is evident that efficient measures cannot be taken to prevent their growth and subsequent dispersion and invasion of the body. It would not necessarily follow that, even if their origin were known, steps could be taken at once, with success, to check, arrest, or prevent, their development. Yet it would undoubtedly be a great point gained to know precisely what to aim at. If it were found, for instance, that the substratum necessary for the due conversion of the [hypothetical] cholera germ into the [hypothetical] cholera poison, is a vegetable, or animal, matter, in a particular state of decomposition in the soil, it might not be practicable straightway to eliminate that matter, or to alter its conditions. But it would, perhaps, be possible to devise hygienic arrangements to meet, or preclude, future choleraic visitations. And in any case a deal of fruitless labour would be saved. Therefore the finding certain vegetable parasites in the body in zymotic diseases, is but the first step, though an important one, in the right direction. Until the tag is put on, as Dr. Salisbury put it on the "ague-plant," search is only in the inchoate stage.

69. In hunting through modern periodical literature for any recent light upon the causation of dysentery, I was somewhat startled and perplexed, at first, by falling across the subjoined paragraphs from the New Sydenham Society's Retrospect for 1869-70. They precede the extract, already given, relative to Dr. Dyes's discovery.

"Pfeiffer [*'Zeitschr. f. Parasitenkunde,'* i, I] gives a historical review of the occurrence of dysentery in Thüringen. Hufeland, in the epidemic of 1795, had already recognized the contagious principle in the stools of dysenteric patients. No accounts of any outbreak since the widespread epidemic at the end of the last century exist. In 1868 dysentery was so prevalent in Weimar that about 1200 in a population of 15,000 were attacked, and at least fifty died. The epidemic commenced June 15, and gradually died out towards the middle of September. It was most common in the narrowest and most thickly inhabited streets, and appeared first in the same districts of the town in which typhus (typhoid ?) and cholera had occurred in 1866, though it was by no means confined to them."

"Hallier (ib., 71) was supplied by Pfeiffer with the necessary materials for his investigation, and claims to have discovered the parasite which is the cause of dysentery. The vegetable growth found in the stools of patients affected with this disease is so similar to that found in the evacuatious of cholera, typhus (typhoid?) fever, and other infectious diseases the chief seat of which is the intestine, that it would be impossible to employ it by itself as a means of distinguishing these affections. At the same time, he holds that the spores found in the diseases mentioned have a different origin. Experiments made by planting prove that the parasite found in dysentery is the sporule of an entirely distinct fungus, not obtained by the cultivation of any other micrococcus, and apparently unknown to most observers. Whether it be the cause, or only an accompaniment of the affection, can be settled by experiment alone."

70. This announcement that Hallier formally "claims to have discovered the parasite which is the cause of dysentery," was rather disconcerting: for it looked at first sight as though the whole train of the causation of the disease had been opened up. Hallier had arrived, *per saltum*, at what the world had been labouring at in vain for centuries. It seemed as though the solution of the problem which I had just compassed, had been found out and already appropriated by the microscopist; and, though I could not decently begrudge him the discovery for humanity's sake, yet it will be granted, perhaps, that it was slightly vexing that after all these thousands of years during which no one had divined the cause of dysentery (except Moses), two men should have hit upon it independently of each other and by different methods, within a couple of years, and that I should be the second in point of time. I thought of the two astronomers and their one star.

71. My first impression was to drop the subject and leave it to Hallier; especially as it was altogether foreign to my pursuits. But upon examining the position more carefully, it occurred to me that possibly the micrococcus Hallier had discovered might not turn out to be the true dysentery germ after all; and that, even if it were the actual germ, he might not have succeeded as yet, and might never succeed, in tracking this fugitive cryptogam to its lair. Many other fungous growths had been made out, but they were still but hypothetical germs of the disease in which they had been found. Again, the echo of the actual discovery of the dysentery germ would have reached even the Antipodes in two or three months; and though I could not make out the exact time when Hallier discovered these sporules, it was clear at any rate that their cultivation had not borne available fruit by the time of the Franco-Prussian war. For in the *Lancet* of November, 1870, is an interesting account of dysentery and typhoid fever at the seat of war, written by Dr. Wibel, Physician to the German Hospital at Nancy. That dysentery had not been averted in the German camp, and that no special means had been taken to avert it, was proof positive to my mind that Hallier had failed in his endeavours to

connect the fungus he had found with its origin. If he had succeeded, he would have taken in all the consequences of such a discovery at a glance. And, from what we know of the organisation of the Prussians, there would have been but little dysentery—or typhoid fever. All things considered, then, there were strong grounds for concluding that Hallier had not, so far, produced that direct and immediate effect which an acquaintance with the actual cause of dysentery would have enabled him to make. The quick thought of a ready means of prevention of this disease would have trod upon the heels of the knowledge of its causation. The one cannot be learnt without the other following closely. Therefore as Hallier had not shown how to prevent this affection, or lessen it during warfare, I felt he could not have been possessed of the secret of its origin. So I took heart of grace again, and went on with my rough work with the uncouth materials at hand. If Hallier, with his finer and more delicate implements, shall have succeeded before me in evolving the same cause—or another efficient one—of dysentery, I shall heartily wish him joy:—though it is superfluous, for I know the feeling he will have had when the thought of the full effect of his discovery breaks upon him.

72. Although it may not be prudent to hazard theories as to the precise mode by which the dysentery germs are developed and scattered in the air—for upon a point of this nature all one's notions can be, at best, but guess-work at present—still I have no such morbid dread of being shown to be wrong hereafter, in matters of pure speculation, as to be deterred from risking views which I conceive to have, possibly, something in them. I have no especial sympathy with that extremely cautious order of men who are everlastingly afraid to air a thought, lest peradventure it may some day turn up to their discomfiture. Right or wrong I shall strike. If right, it's something done. If not—what then? One is in a glorious majority, and there's a goodly company to keep one in countenance.

73. In the first place I may observe that the microscope is not essential to the proof of the theory here advanced as to the causation of dysentery. I affirm confidently that if this marvellously valuable aid to investigation had never been discovered, it would still have been perfectly feasible to have established the correctness, or to have shown the unsoundness, of that theory. The views I hold admit of proof, or disproof, by much more ready, practical, and coarse, yet thorough and convincing, tests. Any man of ordinary sagacity can devise means to put such simple propositions as are submitted to an *experimentum crucis*; and two or three years would suffice to settle their fate without the intervention of the microscope. There is no absolute necessity, therefore, to invoke the assistance of this instrument to determine whether or not fœcal matter is the only source of dysentery. Yet it would be folly to discard so useful an ally—especially having in view the causation of other diseases. Though it might be possible to demonstrate the broad fact that dysentery depends

solely upon excrement for its endemic and epidemic outbreaks without microscopical research; yet there are many obscure points in connection with the origin of the disease, that can be elucidated and finally disposed of only by the microscope. On this elaborate combination of lenses we must depend for complete enlightenment, not only in dysentery, but as regards the remote causation of most other diseases.

74. In fact I believe the discovery of the cause of this very disease could not long have been delayed. I suspect the microscopists are on the high road now, and a very few years or hours might suffice to lead them to it. Supposing that I have blundered on the thing, I shall only regard it as an anticipation of their work and feel that it has been snatched, as it were, from before their eyes. If the micrococcus of Hallier should be the veritable germ, and its nidus should prove to be the one I suggest, it is most improbable that that nidus should long have escaped detection. Either Hallier himself, or some other shrewd observer, would infallibly have hunted it down. There was no help for it.

75. There are two principal modes by which fœcal matter, under the conditions specified, may be supposed to cause dysentery. One is that the putrefactive changes, or the fermentative processes, it undergoes, give rise to the formation and evolution of poisonous gases, or mephitic exhalations; which gases, or exhalations, mixing with the atmospheric air in certain volumes, or proportions, are received into the lungs and produce the affection. The other mode of causation is this:—that fœcal matter after rain, or in damp or moist weather, is invaded by cryptogamic fungi (say) of the *Physomycetous* kind—or in other words is covered with a kind of mould, or mildew; that these plants pass through their successive stages to fructification; and that then the sporules and other minute portions of the plants are disseminated by currents of air during the day, or are suspended in the atmosphere by the dews of night—as in the case of the "ague-plant" (66). Of these two presumable modes by which dysentery may occur from excrement, I am disposed to discard the first for many reasons which need not be gone into just now. The second, I am inclined to think, is somewhere near the mark—if it has not hit it.

76. Most men of observation will have noticed the difference between the mildews on the dung of animals. The microscope demonstrates the specific characters of most of these various fungi, which, however, do not appear to have been studied with that extreme care and minute attention which have been displayed in investigating some classes of cryptogamic plants. Very little, in fact, seems to have been clearly made out in this singularly important, though not inviting, field. I have hazarded the view that the mildew on the excrement of man which causes dysentery, is one of the *Physomycetes;* but I am not prepared to say that it may not be one of the little known genus *Sporotrichum,* or some other of the largely distributed tribe of *Hyphomycetous* fungi. It seems to me more than probable, however, that the dysentery germ, if of

fungous origin, comes of some *Hydrophora*, or *Mucor*, or *Pilobolus*. But the whole of this branch of Mucology is involved in so much obscurity, that there is no knowing whether the dysentery mildew may be one of these, or a *Xylaria*, a *Hypoxylon*, an *Agaricus*, or a new and distinct fungus. It may never have been described, or observed.

Of the germs *Hydrophora*, only two species have, apparently, been defined in Britain—the *H. stercorea*, and the *H. murina*. The first of these is described as being common on the dung of animals after much rain; and the second has been shown by Fries to be peculiar to the dung of rats. (Sowerby names this *Mucor fulvus*.)

With regard to the gernus *Mucor*, there seems to be great doubt whether the last should be separated from it. Its most common species is the *M. mucedo*, a form of mildew not unlikely to be the active agent in much mischief to animal organisms. There are many other species of *Mucor*, having their homes on decaying fungi, and rotten pears and gourds; but the only one that need be referred to is the *M. caninus*. This is said to be very common on the excrement of dogs and cats in very wet weather. [It is to a species of this genus, by the way, that Hallier ascribes the measles germ. In the translation of Niemeyer's Text-Book the fungus is given as the *M. muado*; but this I take to be a misprint for *M. mucedo*.]

The genus *Pilobolus* seems to have received but little attention, except from Cohn, whose works I have not got. The most I can learn is that the *Piloboli* are little moulds growing upon dung. Cohn would seem to have studied *P. crystallinus*, but I know nothing of its nidus.

Of the other genera named, I find but little more than the names and some of the habitats of the species given. Thus I find such notices as follows: *Xylaria pedunculosa*: "on soil mostly attached to dung—not common." (Berkley). *Hypoxylon coprophilum*: "on dung." (Fries). *Agaricus coprinus Hendersonii* (Seeman) "horse dung. This I have seen only once in Crackly Wood." *A. chioneus*: "wood or dung—rare." *A. ovalis*: (Fries) "on dung—rare." *A. albo-cyaneus*: "on dung—not uncommon." *A. stercorarius*: "on dung." *A semiglobatus*: "on dung—said to be poisonous—extremely common." (Berkley). *Coprinus*: "deliquescent Agarics growing for the most part on dung in all parts of the world." (Hooker) *C. Colensoi*: "on dung." *Onygena felina*: "dung of cats." *O. corvina*: "raven's dung." *O. equina*: "on decayed hoofs of horses." *Ascophora elegans*: "on fowls' dung." *Aspergillus dubius*: "rabbits' dung." *Botrytis Jonesii*: "on dung." *Peziza stercorea*: "cow and horse dung." *Sphæria stercoraria*: "sheep and horse dung." *Ascobolus ciliatus*: *A. furfuraceus*: *A. glaber*: *A. carneus*: all on cow dung. *A. vinosus*: rabbit dung. *Poronia punctata*: "on horse and cow dung—not uncommon." *Sclerotium stercorarium*: "dry cow dung." But I need not specify the remainder:—a score or so more connected with dung would perhaps include all the species made out. Mr. Archer and Mr. Gunn, of Tasmania, by the way, contribute to the subject, I observe. The former botanist has a distinct *Mucor*—[*M. cervinoleucus*—Berkley.]—found "on the dung of some small wild animal." He also found a peculiar mildew on the dung of the *Phylacinus Harrisii*, or *cynocephalus*:—the Tasmanian tiger, or hyæna. The Baron von Mueller has not yet arrived at this division of the botany of New Holland in his great work. When he does come to it, there can be no doubt but that mucology will be enriched with new, rare and interesting mildews.

77. Making all due allowances for the information contained in the recent valuable monograms upon the fungi which are not within my reach, and for such late contributions to this especial branch of enquiry as I can know nothing of, it is yet perfectly clear to my mind that this particular department of cryptogamic investigation has not been attended to with that care which has been bestowed on many others. I have not had over much time to search diligently, it is true; but from such occasional glances as I have been enabled to throw over the works on this subject, I conclude that this wide field has scarcely been entered on by microscopical and other labourers. As I confess to being somewhat in the dark, I may perhaps be forgiven if in the following remarks I should inadvertently enter upon ground that has just been broken by others. Those who write with scant materials at this side of the world may claim some slight indulgence on the score of their distance from the centres of enlightenment.

78. It strikes me that these vegetable parasites on excrementitious matters play a much more important part in the economy of nature than has hitherto been supposed, and I strongly suspect that some obscure diseases both of men and animals may be more intimately connected with these minute fungous growths than is now believed. The very slight amount, and extremely unimportant nature, of the information I have been able to collect, convinces me that the scientific world is not alive to this source of danger. The subject indeed has not been studied with anything like attention. One or two observers have examined into one or two species or genera, but there has been no large comprehensive survey of these peculiar mildews. There is nothing to show whether any particular kind of excrement passes through successive changes, during which it gives rise to successive crops of different mildews at its different stages of decomposition. I cannot learn, definitely, whether two or more species of cryptogam may and do inhabit the same mass of dung at one and the same time. There is no positive indication whether or not any of these fungi have any inherent poisonous or peculiar qualities; and, if so, whether they derive them from the nature of the material, or from the state or condition, of the substance, upon which they grow. Nor is there, of course, any account to be found of the various pathological effects that are produced upon animals by the introduction of the spores, sporanges, pedicels, columellæ, or any other parts of any of these mildew plants. The meagre accounts attached to the few genera and species I have named, contain the sum and substance,

apparently, of what is known respecting these forms of vegetation, and the erudite seem to have doubts whether even that little is sound. Thus it is a question whether *Hydrophora* should be separated from *Mucor*.

79. Where there is such a dearth and poverty of definite knowledge, I shall not hesitate to express my conviction that there has been an absence of that thoroughness of enquiry which is necessary to place this subject on a satisfactory basis. Nor shall I refrain from saying that I take leave to doubt the perfect accuracy of some of the descriptions given—not as to the specific characters of the plants themselves, but with regard to the kinds of excrementitious matter on which they are stated to be found. The accounts as to their habitats are too vague, loose, and general, to be of real scientific value. For instance, when we are told that the *Mucor caninus* is common on the excrement of dogs *and cats* in very wet weather, the information is all but useless; whereas if the observations had been extended a little further, they might have proved of great interest. If this *Mucor* is found to be common to the dung of dogs and cats, the question arises whether it is common to these two kinds of dung exclusively, or whether it has an equal facility in extension to other dungs in the neighbourhood. If the *Mucor caninus* spreads to any kind of dung under certain conditions, it does precisely what every other mildew will do under certain conditions. But if it selects these two substrata, and refuses to invade all others, it is a very remarkable mildew indeed. If that were the inference to be drawn from the statement as to the habit of this fungus, I should want very strong evidence as to the fact. But the remark is clearly made without any definite object or meaning. It is the mere chronicle of a thing observed without any thought of other things, unobserved, but related. The probabilities are that the *Mucor* in question is the specific mildew of the dung of one or other of the animals, but that under favouring conditions it will spread to other excreta—becoming modified, however, and losing some of its original characteristics.

80. For without data, but by analogy, I infer that the mildew originating on the excrement of all animals of different genera is a specific mildew; and that the components of the excrement determine the kind of cryptogam of which it shall be the nidus. I suspect it will eventually be found that the fungus originating on the dung of the cat, is as distinct from that originating on the dung of the dog, as the latter is from that on the excrement of man; and that the cryptogams of all three not only differ among themselves, but that they differ from the cryptogams of the excrement of all other animals having essentially different alimentary conditions. I do not mean to affirm that there may not be fungi common to very many forms of excreta, if not to all. That commonest of all moulds, the greenish or bluish *Penicillium*, has a remarkable facility in adapting itself to any organic matter. It, as well as other mucedinous fungi, and perhaps all the *Mucorini*, will spread their mycelia over dung readily, independently of its

composition, under favouring circumstances. Some one or more of these fungi may coexist with the specific fungus, or they may precede, or follow, it, according to seasonal or other conditions. It may indeed happen that these extraneous conditions may prevent the development of the characteristic or specific cryptogam of a given dung. It may fail entirely. But what I contend for, and what I believe will ultimately be brought out by more extended enquiry, is that in addition to these ordinary fungi, which may affect many kinds of excreta indiscriminately, the dung of animals having distinct generic differences will be found to have at some period or other their specific cryptogams. Where there is a well marked distinction in the excretions, there will be as well marked a distinction in the mildews. To give one illustration;—which may not, however, strike the reader so forcibly at this stage, as it does me—whatever may be the specific fungus that produces the dysentery germ, it is clear that it cannot be developed by the fœcal matter of any animal but man, or the civilised world would never be free from the disease. But this, I am aware, is to beg the question. It has not been shown yet that the dysentery germ is a cryptogam arising out of human excrement. I will therefore fall back on the only other decided exemplification that science supplies. The authorities appear to be agreed so far, at all events, that there is a mildew on the dung of the rat, distinct from that on the dung of the dog, or cat. And if this be granted, I claim a distinct mildew for every as distinct kind of dung. I do not know whether the principle will be granted, or not, but as the question is only a collateral one I shall not stop to discuss it in all the forms; though, as it is an extremely important one and bears indirectly upon the causation of dysentery, I will offer a few observations that may serve to support the view I take.

81. That there is a wide difference between the dung of different animals, it does not require the microscope, or any elaborate scientific process, to assure us. Everybody must recognise at once the vast distinction between the sweet wholesome smell of an old cow-yard and the stench of a room in which a cat has been forced by circumstances to leave her excrement. There can be no mistake in this. Then again there is as evident, if not so strongly contrasted, a difference between the odour of a cow-yard and that of the pungent ammoniacal sheep-fold. The stable, too, has it characteristic effluvium, clearly distinguishing it from the pig-sty. If observers have hitherto omitted to find specific differences in the mildews of the dung of the few animals named (sufficient however to represent every type of animal in existence), I must assume it is because they have not searched for them, rather than that they do not exist.

82. The popular belief is that a cow-yard has an actual salutary effect on the human body. Whether this be so or not, it may very fairly be inferred that the mildews, whatever they are, *Pexiza*, *Ascobolus*, &c., occurring on decaying cow dung, have no known or marked deleterious effects on the system of man. Large

collections of these excreta do not, seemingly, give rise to any empoisonment of the atmosphere with fungi that act injuriously on the blood or organs of man (at all events, in the first instance). But now how about the cat? Do not all the surroundings of the excreta of this animal point most unmistakably to danger somewhere in leaving them exposed? Why does the cat, of all our animals, cover her excrement so carefully? Is it supposed that there is not some significance in this curious habit? Where is the necessity for resorting to this natural and effective earth-closet system? When this singular provision is taken in connection with the well known repugnance of this animal to defecate in a room, or wherever it is unable to follow its ordinary habit in this particular, one is forced to conclude that there must surely be an instinctive feeling, or knowledge, of some highly noxious or poisonous results, probably to the cat itself, if its excrement be left without the covering of earth. I know nothing of the matter —in fact it has only just occurred to me now in writing—but, as it is not in accordance with natural laws that anything should be purposeless, I suspect the cat has a very cogent reason indeed for taking so much pains to cover her dung—a far stronger motive than the possibility of befouling herself in the future. It may be that some philosopher has investigated this phenomenon and has demonstrated the reasons for this unique instance of hygiene among brutes. If so, I have not met with any account of it.

83. My impression is that if the dung of cats were collected, and placed under favourable conditions for the development of whatever mildew may be peculiar to it, [the *Onygena felina* perhaps] and cats were then brought within the sphere of its influence, it would very soon be seen why they are so anxious to cover their own excrement with the antiseptic earth. I surmise that an experiment of this kind would bring out some curious results, and might possibly show that the sporules of the specific fungus which may be given off from the mildewed dung, are the dysentery germs of cats, or cause some analogous form of intestinal exanthem—feline typhoid perhaps. Something disastrous to the system of the cat, I feel persuaded, would follow its being compelled to inhale an atmosphere loaded with emanations from its own excreta. The probabilities are too that the mildew in the case of cat's dung forms more rapidly than that on other excrements; for the reason that it is of a semi-fluid character. It contains a larger proportion of water than the dung of most animals and is, therefore, less dependent on atmospheric conditions for the evolution of its fungi. It is markedly contrasted in this respect with the *alba canina;* and, in fact, the excretions of the dog and the cat differ so much in their physical appearance, that I cannot believe the *Mucor caninus* is common to both, or rather that each excretion has not a special fungus *sui generis.* Besides the probable deadly effect upon the cat, it is not impossible also that its mildewed dung may have some noxious effect upon man; and if it were left about dwellings, uncovered, might lead to mischief.

What about the other felines by the way? Do they follow the same habit? And have their excreta the same degree of fluidity? Do tigers, leopards, lions, panthers, &c., in confinement, show any desire to cover their excrement? Do they go through the useless form?* It would be interesting to learn whether at the Zoological Gardens any accumulations of excrement have ever taken place in the dens of these animals, from the neglect of keepers, or from their inability to remove the excreta; and, if so, whether any perceptible effect upon the health of these felines has followed. And what becomes of the collections of feline dung? Have the keepers of menageries, or others, any experience as to obscure, or strange, diseases? This allusion to wild beasts reminds me of the vast field the Zoological Gardens open up to the student of mildews. How many new species and genera may he not find on the excreta of the camel, the elephant, the hippopotamus, and the hyæna! But perhaps the most singularly interesting of all will be the mildews on the fœcal matter of the monkeys. Can dysentery germs, or typhoid, or cholera, germs, be developed from these? Would it be possible to demonstrate the soundness of the theory submitted as to the cause of dysentery, by subjecting quadrumana to the effluvia from their own mildewed excreta, or from that of man? Judging from the points of resemblance between the higher apes and man, and from the analogy between their internal organs, there are fair grounds for assuming that the glands in the large intestine of apes are somewhat similar, if not identical, in structure, with the human glands in the same position; and that they perform the same functions. If this is so, then there is no reason why apes should not be as subject to dysentery as man, and from the same cause—the development of a dysentery germ from the same matrix.

84. Has dysentery ever been known to occur among apes in Europe? In England, where epidemic dysentery has disappeared, it is not so likely to have been observed. But on the continent, where there are large collections of animals, and where dysentery is still prevalent, the disease may have extended to the apes. When the "frightful epidemic," of which Trousseau speaks, occurred in Paris in 1859, was it found that there was any unusual mortality among the apes in the *Jardin des Plantes*, or elsewhere? And if so, was the complaint of which they died taken any note of? Did it resemble dysentery? There will surely be some records of these things somewhere, though I cannot find any account of them, or it may be that personal recollection as to the fact may be forthcoming. If it should transpire that these creatures shared the fate of the higher grade, and perished from a bloody flux, it will be a most valuable aid to the furtherance of future investigation. It is

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* Since writing the above I have ascertained a curious fact with regard to the habits of the lion and lioness in the Royal Park, Melbourne. It appears that the lioness invariably scratches the boards of the den after voiding her excreta, in the same manner as the cat; but the male animal makes no useless attempt to cover his excreta.

not impossible, however, that the apes may have escaped the epidemic, from the fact that they would have been protected by their position to a very great extent. Yet the chances were against them notwithstanding, and it will not surprise me in the least to learn that they suffered equally with the Parisians.

Another thought suggests itself. Can the excreta of apes or monkeys produce the dysentery germ of man? Is it possible that in the countries where there are countless troops of these animals, the air is ever rendered poisonous to the human race by their mildewed excreta at rainy seasons? Or can any of the sporadic cases of dysentery that occur in London, be accounted for by any infection from such a source? Have any of such cases occurred to any of the persons employed about the Zoological Gardens? It is quite easy to conceive that monkeys kept by private persons, and allowed a certain amount of liberty, may hide their excreta, or leave it where it is not perceived about their kennels or sleeping places. If the excreta are capable of developing the dysentery germ, and if they are left in the open air and are subject to rain or moisture, there is a cause of idiopathic dysentery.

85. These disjointed thoughts have sprung out of the subject of the mildews on the dung of foreign animals. They are merely random shots which may hit something. It is possible that some enquirer may get a hint, and that is all intended. If Zoological Gardens can be utilised in such matters as have been glanced at, their value will be marvellously increased. To return to the tame beasts of Europe.

86. Perhaps it may be from having conceived the peculiar views I entertain as to the cause of dysentery, but I have a half-formed, cloudy kind of notion, that the low vegetable growths which affect the dung of animals are more especially obnoxious to the animals from the dung of which they spring. The idea is not matured so far as to enable me to make out a presentable hypothesis, and it may be weak to expose bare one's thoughts. Yet as this speculation on germs is after all, but a jumble of crude ideas, I do not know that I need hesitate about this one. The main objection I have to giving it expression, is, that I have so little evidence to advance in support of it and have no prospect of obtaining more for some time to come. However, it will be understood that these views are merely *disjecta membra* of unfinished hypotheses.

87. The view is that the dung of every animal has the inherent property of producing, under special circumstances, certain vegetable organisms which are more particularly deleterious to the animal from which it comes. This does not exclude the idea of other results as regards other animals. The fungi from the excreta of one class of animals may, and probably do, find a substratum in the bodies of other classes of animals, under peculiar conditions. But they do not produce the same specific effects; but other specific effects, varied by the substratum, or the body into which they find their way. There is the bare possibility that some fungi, following the analogy of certain animal parasites, require to pass

through, or over, two or three substrata before they reach their highest stage of perfect development. In their successive steps they may cause specific effects on the living bodies through which they pass. As with entozoa, so with fungi, very rare combinations may be required to produce some forms. It is even possible that the world has not yet seen, and may not see for centuries, all the complications arising out of unprecedented conditions being thrown together. There may be unborn plagues for man and beast in the womb of time, only awaiting some hitherto delayed event in the parasitical kingdom to be brought forth. But, to leave remote contingencies and the mysteries and obscurity which at this moment veils the origin of some established diseases, I think it is possible to trace some actual and tangible, though indefinite, relation between the dung of animals and their diseases.

88. The first animal I select to illustrate my meaning is man. We know more of him necessarily than of other animals. If a clear and undoubted case be made out as regards him, he may be accepted as the type of all others; for the same physical laws of nutrition run through the whole series. I submit that *dysentery* will be eventually brought home to fœcal matter. I shall give hereafter such reasons for connecting both *typhus* and *enteric fever* with the same source, as satisfy me that they are dependent on it for their causation. Probably also *relapsing fever* is connected with human excreta. Then there is *yellow fever*, which I take to have started entirely through some combination of factors in which fœcal matter played the chief part, and to be wholly dependent, now, for its maintenance and propagation, upon human excrement exposed under such conditions as are principally found in tropical climates among certain populations. And then there is *cholera*, This is doubtful ground I know, and occupied as it is with all the acutest intellects of Europe, it may argue extreme temerity to step upon it, and to presume to obtrude a thought. I shall do so, however, with becoming respect to the great minds that are now agitated with this complicated problem. I have gone through all the proceedings I could get of that body of representatives of the *élite* of the medical world, which has met at the various great capitals for some years past. And it is impossible not to admire the active measures taken, and the ingenious theoretical views discussed, by what may be called the Cholera Parliament. I have studied too the remarkable, elaborate, and deeply interesting theory of Pettenkofer in its epitomised English dress. And I have dipped into as many of the writers on this disease as my limited time and means have permitted.

Taking all these sources of information, and viewing the question of the causation of cholera from my present stand-point, I declare boldly for its purely fœcal origin. The subject is too large to stop to discuss here, and my data are too meagre to do anything effective with them perhaps; but when I come to treat of disinfection, I shall have a few words to say which may tend to show that there is very little chance of preventing the periodical cholera wave from overwhelming Europe in the future, so long as the races of Hindustan shall follow their present customs, and shall leave the excreta of about a hundred, or perhaps a hundred and fifty millions, of people, daily, on the surface of the ground. Not only can dysentery never be stamped out of a country inhabited by such a people, but cholera, I strongly suspect, can never be kept down in the country itself, nor prevented for many years to come from spreading through the world by its present channels of communication. Hygienic measures may prevent the due ripening of the cholera poison (taking Pettenkofer's view) here and there; but I predict that nothing can be done to stop this deadly plague short of scavenging India by some method—a gigantic piece of sanitary work, truly, but the only real remedy that I can see. Whilst the holy custom of the Hindoo and the Mussulman referred to shall last—and it has existed now for some thousands of years—long before Mahomet's time—there must ever be a wide-spread matrix on the face of that enormous tract of country, ready, when seasonal influences shall be propitious, to give birth to and to propagate myriads of those cryptogamic plants which, I believe, supply the mortal cholera germ. Not that all India is capable of originating this fungus I am aware. It seems to exact certain local and seasonal conditions for its development. And it strikes me that this peculiarity as to the foci from whence it radiates, should determine that the next, or an early, meeting of the Cholera Parliament should be held on the spot, rather than at Constantinople, or Vienna, or St. Petersburg, or London.

89. No less than six dangerous and largely fatal affections are here enumerated as having their origin in the excreta of man himself; and there are others not considered. It is not asserted that any of these diseases are as yet proved, beyond doubt, to be caused by this means; and it would manifestly be impossible for one man to do all this single-handed in a few months. Yet I undertake to prove the direct connection between fœcal matter and dysentery before I have done. I maintain that the chain of evidence I shall adduce cannot be logically shaken, and will not be experimentally broken. If this be established—if it be finally proved that I am right as regards the causation of dysentery—the causes of the other diseases must be made out soon. For the present I will content myself with assuming that the dysentery germ comes from fœcal matter. If man be shown to be liable to disease from his own excreta, it follows that other animals may be also liable to disease from their excreta. And herein I fancy I detect an explanation of some of the severe epidemics which now and then spread dismay among the cattle-breeders and flock-owners of the world.

90. To begin with horned beasts. It may be quite true that the savour of a cow-yard is not unpleasant, and that it may even be wholesome to man—(in the direct sense and in the first instance). Granting this, it yet does not follow that the mildews that may form on the dung of cattle are equally harmless and

innocuous, under all circumstances, to the cattle themselves. The excrement of man does not produce the same specific effect on other animals [apes perhaps excepted] which it produces on himself. Nor need cow-dung produce the same specific effect on man that it produces on cattle, in order to establish that it does produce specific effects upon the latter. It is not a consequence that, because cattle do not contract dysentery from mildewed human fæces, men do not get it from that source. Nor is it a consequence that, because men have not been affected with the foot-and-mouth disease from inhaling the air of cow-yards, cattle do not get it from that source.

The first thing that naturally occurred to me in following up this train was to learn, if possible, what herds of cattle do when left to themselves on a sufficiently wide expanse of country to afford them a good chance of selecting their own camping grounds at will. I found what I suspected. One or two owners of large cattle stations in Australia, men of acute observation, have told me that particular "mobs," or herds, of cattle have favourite spots for camping on, and thither they all resort at set times and whenever they are disturbed by "mustering," or what not. But although the same cattle always take to the same spot, it appears that they do not occupy precisely the same area of ground for more than a week or so at a time. "They shift about from place to place, but in the same neighbourhood." Assuming this to be correct, and I have every reason for believing it is, there is an indication that horned beasts do not voluntarily subject themselves, in a state of nature, to the effluvia from their own excreta. And this, it will not be overlooked, in one of the most arid countries under the face of the sun—a region the least favourable for the production of mildews. This little bit of natural history is as suggestive and as significant, though not so striking, as that connected with the custom of the cat. It points to an instinctive fear of the effects of inhaling an atmosphere laden with cryptogams from their own dung; and I suspect that if the habits of cattle in a comparatively wild state in moister countries could be ascertained, much more decided and marked aversion to old camping-grounds would be shown.

Although from what we know at present animal parasites would seem to be more largely concerned in the diseases of cattle than vegetable fungi, yet I have a conviction that some of the obscure diseases periodically affecting horned beasts epidemically, for which no satisfactory, or efficient, cause can be assigned, will eventually be traced to the mildews on their dung; and not, as suggested by Hallier, to the cryptogams growing on plants. I observe that this indefatigable microscopist has been at work in this field too. He procured from diseased animals a fungus to which he has given the name of *Coniothecium stilesianum;* and he suggested to the scientific world of the United States, which has been busily investigating the causes of "the periodic fever" of cattle and of the "lung plague," that search should be made in the food of the beasts for this parasite. The result appears to have been that the American *savans* not only did not find the *Coniothecium*, but they threw out strong doubts as to these fungoid growths being concerned in diseases at all. They even challenged the cholera germ and all the specific germs of fevers! As, however, the cholera germ was voted an existence by the Cholera Parliament, and as Dr. Salisbury's "ague-plant" has not been shown to be a myth, so far as I know, I must still assume that the German school is right as to facts observed. Perhaps both the "periodic fever" and the "lung plague" may owe their existence to animal parasites after all. If the latter be the same as pleuro-pneumonia, the elaborate researches and ingenious views of our Victorian microscopist, Mr. Ralph, [which have been so highly spoken of by the great English authority—Cobbold] may throw some light on its causation. At any rate I am not prepared to give up the cryptogamic germ theory at present. And I suggest that if the foot-and-mouth disease cannot be assigned a cause by any other means, it may not be time thrown away to investigate the subject of mildewed dung as a possible cause. Then again there is the cow-pock; that mild exanthem which first suggested to the immortal Jenner the idea of vaccination. This disease of the cow seems to me singularly likely to occur from cow-dung. And if so, and bearing in mind the close connection between this affection and the severer disease of man, who can tell that there may not be some relation between the origin of small-pox and the occurrence of these fungoid growths on cow-dung? This may seem idle to what is called a practical cast of mind; but I can conceive immense benefit to mankind to be derived from closely investigating such a subject.

Before quitting the mildews occurring on cow-dung, I would draw attention to the pregnant fact, that no less than nine forms of mildew have been described by mucologists as having been found on this one substratum: viz., *Ascobolus ciliatus; A. furfuraceus; A. glaber; A. carneus; Peziza stercorea; Poronia punctata* [on horse and cow dung]; *Sclerotium stercorarium* [dry cow dung]; *Coprinus stercoreus;* and *Cyathus Colensoi.* Others, which I may easily have missed, may also have been described; and others may be developed which have not been found. However here are at all events nine separate and distinct fungi, eight of which are ascribed to cow dung only and one is common to it and horse dung. Several very important considerations arise out of this singularly large group of mildews. In the first place one is led to enquire how so many species came to be observed on this particular dung. Have the cryptogams of cow-dung received special attention from mucologists? Or do the excreta of horned cattle develope a larger number of different fungi than the excreta of other animals? As regards the first question, it is not unlikely that the notice of observers has been more attracted to the vegetation on this kind of dung than to that on other kinds; for it is perhaps more largely distributed over the surface of Europe than other kinds of excreta—in the matter of bulk especially. The size of the evacuations, together with the comparatively small repugnance that botanists would have in examining this substance closely and minutely, may have led to a more searching investigation of its parasites than has been made in other directions—that of human excreta for instance. There seems no good and sufficient reason for supposing that the dung of horned beasts developes more fungi than the dung of other graminivores. And though this department of the economy of nature has not been entered hitherto, and we know literally nothing of the work going on there, I do not see, at this moment, why the excreta of the carnivora should not be as fertile in the production of fungi, as those of the graminivora. Taking the greater variety of the *ingesta* of man and other omnivorous animals into calculation, I should be disposed to conclude, on general principles, that their *egesta* would evolve and support a larger number of distinct fungi than the excretions of any other class of living things. If the dung of cattle in Europe has been shown to produce no less than eight kinds of vegetable parasite, it seems reasonable to suppose that the dung of other grass-feeders in Europe should each and several produce an equal number; and it certainly would not seem an unreasonable supposition that the excrement of man, throughout the world, should be capable of producing at least the same quantity of specific fungi. When the matter comes to be really examined, I suspect that many more will be found. But for my present purpose, eight will do, or I might even say half the number. Assuming human excrement to contain elements capable of generating no more than four specific mildews in Europe;—what are those four? Has anybody found and described them? And if so, has any further account been given of them than that they occur on human excrement? I devoted some considerable time to poring over the collection of works in the Public Library of Melbourne, and a learned botanist joined me in the search there; he also kindly undertook to look through such authorities in the Parliamentary Library as might not be in the Public Library. The end of it all was that we succeeded in finding a brief reference to two *Agarics* on excrement! There was not a single word of any other form of vegetation peculiar to human fœces. Moreover I set enquiries on foot with a view to bring out whether any recent investigations had been made in this especial line of study; and if so with what result. From all I could gather, I think I am justified in saying that mucologists have not yet discovered, or described, one specific mildew belonging to the excrement of man—unless the two mushrooms alluded to may be classed as specific mildews. If I am correct in this inference—though I trust I am not, and that some observer has already entered upon the subject unknown to me—I have no hesitation in saying that the field of botanical research, the most important, perhaps, to the human race, has been left as yet unexplored. The reasons for this may be obvious, but the fact is nevertheless deplorable. For it is no unwarrantable assumption that, if the various mildews which may exist on excrement had been studied, their possible connection with the diseases of man might have been suspected and traced long since. This, however, is somewhat premature at this stage of the enquiry; though its force may be admitted further on.

91. It is unfortunate that the mucologists who have given the names and descriptions of the plants they have found growing upon dung, should have added little more in most instances than the kind of dung upon which the vegetation was discovered. Taking cow-dung, for instance, it will be observed that the only departures from the ordinary account are in the case of the *Poronia*, which was got on horse and cow-dung; and the *Sclerotium* which is peculiar to dry cow-dung. But even these little additional pieces of information, short, curt, and insufficient as they are, are highly suggestive and lead to many very interesting enquiries upon which weighty conclusions may depend. The fact of *Poronia* being found upon both dungs, opens up the great question as to how far vegetable parasites commencing upon one substratum will spread to similar substrata under favouring conditions. From the circumstance that this plant is described as growing on the two dungs, whereas so many other plants are described as growing on cow-dung solely, it may be inferred, as a probability, that *Poronia* originates on horse-dung and thence finds its way to cow-dung. The *Poronia punctata* is one of the mildews found in Tasmania by Archer, but that mucologist has followed the practice of his European brethren, and the habitat of the plant stands thus—"On dung (?)" So that neither substratum is indicated. However whether *Poronia* commences on the dung of the horse, or of the cow, is not of immediate consequence to the present argument. It may not indeed originate on either for that matter. The extension of this specific mildew from one dung to the other, proves exactly what might have been deduced from observation of those other mildews which have been more prominently brought into notice, and more carefully investigated—the *Oïdium* for example. The *Poronia* clearly obeys the same laws. As the mildew on the vine, under certain conditions, will throw itself upon laurels and other shrubs and trees in the vicinity, and will there lay hold and flourish exceedingly to all appearance: so this mildew on the dung of the horse will, under analogous conditions, go to the dung of the cow; or conversely as the case may be. This may seem a simple matter to establish, and as its full significance may not be seen at once, I draw special attention to it, as it forms the basis of some large deductions, as regards variations in disease and with reference to certain points in connection with hygiene. But another consideration arises out of the fact of this *Poronia* having been found upon two kinds of dung, and an important one it is. Or rather there are several highly interesting questions springing from it. What is the evidence that *Poronia* commences upon either of the two substrata given? As it has been found upon both, why may it not have started on a third? Who can determine that these two dungs

only will furnish the necessary conditions for the maintenance of this mildew; or that it may not be a foreign invasion passing over many intermediate substrata before it was finally detected on these? As the growth of the *Oïdium* is far more luxuriant on some other plants than on its native vine; why may not the *Poronia* have commenced as an insignificant and scarcely to be noticed mildew on the scanty substratum from whence it emanated, and have then developed into larger proportions on more abundant material? And what is the nature of the modifications which a mildew undergoes in travelling from one substratum to another? Does it lose any of the characteristics it derived from the parent source? And does it acquire any new characteristics in its alien bed? Is its physical configuration changed in any way? And are its juices and specific qualities altered by migration? Are its sporules still perfected as before? Or is the species propagable in its new lodgings by the process of cell-division only, and not by seed? What is the limit of the extension of a mildew occurring on dung originally? Where is the line of substratum beyond which the mildew cannot subsist in any form? Is it drawn at any particular kind of dung? Or will this mildew creep over any kind of organic matter that may be available? If so must the organic matter be necessarily in a state of change from decomposition of some kind? Can a living substratum of any description be made subservient to the maintenance of a mildew derived from dung? It will be some time yet before full answers can be returned to these questions. They involve some of the greatest problems of the day.

92. The few words indicating the conditions of evolution of the *Sclerotium*—viz., "on dry cow-dung"—are also highly suggestive. They leave the impression that the *Ascoboli* and other genera are developed on recent or moist cow-dung. And a host of curious points stand out at once. Under what condition does the *A. ciliatus* occur in preference to the *A. furfuraceus*, or to the *A. carneus?* There must be some special difference to lead to the specific development of the one plant to the exclusion of the other—or do two or more of these mildews inhabit the same deposition of dung at one and the same time? Are all these mildews found on the same evacuation at the same period? Or are they all to be found at successive stages on the same deposit? Can the one deposit of dung produce all the mildews either at once or one after the other? Or is there any seeming connection between the food of cattle and the appearance of the mildews on their dung? Were the mildews found in summer or winter? On what ground were they found—in fields, or in cow-yards? Were the same mildews found alike in isolated patches of cow-dung, as in large collections? Were they all developed upon the dung before rain occurred—or were some of them subsequent to heavy, or light, rains? Which of these mildews is the most constantly found? And which the earliest in point of time after the dung is dropped? And are all these mildews clearly specific cow-dung mildews—or may they have come

from some other neighbouring substratum? Were they observed on any other dung, or other organic matter, close at hand? Have they been found constantly, or frequently, or rarely, and invariably on cow-dung? Unless the researches of mucologists are directed in the future to some such ends as are here glanced at, I apprehend that the chief value of their labours will be the enumeration of genera and species. As for the omissions in the past, I interpret them as evidences of the slight interest taken in the subject, from not perceiving its drift, or its practical bearing upon questions as to the causation of disease.

93. A few words upon the subject of the cow-dung mildews of Australia. They may yet prove of vast moment to the owners of herds in this country. If, as I suspect, some murrains will eventually be traced to the mildews which have been developed in old countries in inordinately large quantities, owing to the crowding of cattle together on small areas for long periods, it becomes a serious question how far the disease will spread in a district when infected beasts may be introduced. What are the conditions for producing an epidemic of a given cattle plague, depending on a mildew, in a country as yet free? Let it be assumed, for the sake of argument merely, that the foot-and-mouth disease is such a plague; that it occurred from the unprecedented combination of a certain set of factors; and that like cholera or yellow fever it is conveyed from its centre to other places by infected animals. Under what circumstances would the disease be most likely to spread and assume the epidemic form in a country into which it is introduced? The answer clearly is that it would be most likely to take root and propagate itself in a region presenting conditions as nearly as possible similar to those of the region from which it was transplanted. Then comes the question as to similarity of conditions; and, to bring this question home to Australia at once,—are the conditions of this country such as to lead to the conclusion that a serious and wide-spread epidemic of the foot-and-mouth disease would follow the importation of infected beasts—always supposing this specific disease to depend upon a specific mildew upon the dung of cattle? On these premisses I conclude that it would be impossible to create an epidemic among the herds of Australia—just as it would be impossible to bring about an epidemic of cholera, or enteric fever, in Yeddo. [200, &c.] The foot-and-mouth disease might, it is true, involve the milking establishments near Melbourne, and might attack those animals which are kept in paddocks for any length of time, or in great numbers. Wherever, in fact, large accumulations of the excreta were to be found, there would be found the disease in corresponding proportions. The amount of dung would determine the amount of mildew, and the amount of mildew would regulate the extent of disease. But taking into consideration the relatively small quantity of dung on the surface of every square mile of this country and the extremely dry nature of the climate, it is problematical whether the foot-and-mouth disease would exist a month in Riverina, unless it

came originally from some form of *Sclerotium*, or other mildew occurring on dry cow-dung:—in which case it would play havock with the herds resorting to the large camping grounds in the interior. And here the practical value of a knowledge of these mildews would be immense. The only information we possess on the subject at present is that obtained from Tasmania. There four forms of cow-dung mildew have been found: viz., *Coprinus stercoreus* (Archer); *Cyathus Colensoi* (Gunn); *Peziza stercorea* (Archer); and *Poronia punctata* (Archer). These, it may be assumed are all wet cow-dung mildews. The fact, however, that these European forms are reproduced in this part of the world on wet, or moist, dung, makes it probable that the *Sclerotium* will sooner or later be met with on dry dung. If, therefore, it should turn out to be the case that the foot-and-mouth disease was caused by a mildew, and that that mildew was a *Sclerotium*, or a species of an allied genus occurring on the dry dung of horned beasts, then the introduction of cattle infected with that disease would undoubtedly be disastrous in the extreme in a country where the droppings of animals become like tinder in a few hours. But the probabilities are that, if the foot-and-mouth disease was the result of a mildew on cow-dung, as I surmise, the mildew was one which was generated on moist cow-dung; and, judging from the imperfect accounts I have seen touching the origin and spread of the disease, on cow-dung that had been allowed to accumulate. If this be so, the danger from imported cattle is reduced to a minimum, so far as this particular disease is concerned. Of course there may be largely fatal cattle plagues depending for their origin on the mildews of dry dung and owing their great mortality to exceptionally dry seasons. But these are probably very rare in Europe, even if one has ever occurred. If a murrain of the kind should arise, however, from some hitherto unheard of combinations of conditions, and should by some unlucky chance be brought hither, it would be difficult, if not practically impossible, to stamp it out by any means. The foot-and-mouth disease was not at all events a murrain of this kind; as may be inferred, not only from its European history, but from the very slight extension of the malady after its introduction into this country. The explanation of the rapid manner in which a disease which proved so formidable in Europe was got rid of here, is very simple from my point of view—though I admit it may be shown to be the wrong one. It seems to me that the foot-and-mouth disease died out in Victoria mainly because there was nothing to support it; though partly perhaps because of the preventive measures adopted. These latter, however, I suspect, would have been utterly ineffectual, if there had been large collections of the dung of cattle along the line of country traversed by the imported animals; and if that dung had offered favourable conditions for the reception, growth, and consequent extension, of the specific mildew which caused the disease. If the pabulum for the mildew had been there, the subsequent steps taken would have been all too late. Had the same thing occurred in any part of England, there would probably have been a different tale to tell. If these same cattle had been landed on any portion of the English coast and had travelled as far inland as they did here, I conclude that precisely similar attempts there, as here, to stop the spread of the disease would not have been followed by a like fortunate result. The fact too that the same malady was just as easily suppressed in New South Wales as in Victoria, the one visitation being in the winter and the other in the summer, points to the same conclusion. All things considered, I am under the impression that the *Coniothecium stilesianum* of Hallier [90], which he believes to be the germ of the foot-and-mouth disease, will not prove to be the representative, in the organism of horned beasts, of a *Sclerotium*, or of any form of mildew originating on dry cow-dung. When that germ shall be finally traced to its source, I hazard the opinion that it will be shown to have come from a mildew on moist cow-dung; and that it is not only an unknown fungus, but, assuming the disease not to have occurred for the first time the other day, a parasite engendered out of conditions that are rarely brought together. It will be clearly understood, however, that these remarks on the subject of the causation of the diseases of cattle are purely hypothetical. The facts known are so few and so little to the purpose, that nothing satisfactory can be done with them in their present state. One truth brought out—one perfect observation made—might overthrow all calculations based upon such meagre data. I believe that I am on the right track, because I fancy I detect an analogy between the causes of some murrains and the origin of the pestilences of man. Yet I know perfectly well the insecurity of the position taken up on such slight materials. There is one point, however, upon which I take my stand. Whatever the germ of the foot-and-mouth disease, I affirm that no sound knowledge of the diseases of animals can ever be arrived at until the natural history of the vegetation connected with their excreta has been thoroughly investigated, and until the pathological effects caused by the introduction of portions of such vegetation into living organisms have been accurately determined so as to be clearly recognised.

94. After what has been said of horned beasts, it is superfluous to dwell on the affections of sheep. The same rule holds good. *Ex uno disce omnes.* Eruptive fevers and catarrhal maladies among these animals I believe will be traced to mildews; and when these diseases occur sporadically, or endemically, or when they are communicated from an infected source, and then increase and become epidemic, it is a warrantable inference that both the origin and spread of such diseases are due to some error in folding, by which the growth of parasitical vegetation is encouraged. In running through mucological works for information on the subject of mildews on excreta generally, I fell in with only one fungus connected with the dung of the sheep, the *Sphæria stercoraria*, [Sowerby] which is described as being found also on horse-dung. I am not prepared to say, however, that more

kinds might not have been found by a more diligent search, or that some others may not have been added to the list recently. But looking at results, it appeared to me a waste of time to wade through all the volumes on the shelves. It was evidently hopeless to expect to find more than a name, a description of a plant and a bare mention of its habitat—with now and then a brief and vague allusion to conditions. There was nothing complete, or solid, or available for inductive purposes. The impression left on my mind, though, is that the parasites on sheeps' dung have not received anything like the attention bestowed on those of cow-dung. The fact that fewer fungi have been found and described on the former than on the latter, does not argue that there is the same relative proportion in nature. Perhaps larger bulk and more moisture may possibly govern the number of mildews to some extent. Otherwise there is no evident especial reason why excreta from grasses passing through the system of cattle, should be more fertile in mildews than excreta from the same source coming from sheep. The probabilities are that if mildews were searched for carefully on sheeps' dung under various conditions, they would be found. And for reasons glanced at before, I should expect that the parasites on the dung of swine would be more numerous than on the excreta of purely grass, or of purely flesh, eaters. Yet strange to say, I did not light on one mildew on swines' dung in the works of mucologists.

95. Why has endemic glanders been apparently almost extinguished in England, and why have epidemic attacks ceased altogether for the last 150 years? It still remains upon the continent, and appears to break out regularly among cavalry horses in times of war. I observe that the topic has been ably handled and I do not know that I have any new light to throw on it, although I am tempted to offer a remark which does not appear to have occurred to others; or, if it has, it has not had sufficient point or significance, perhaps, to be set down. Only that glanders has not been known in Australia up to the present time, I should have been under the impression that there was an alliance between it and dysentery. And even now, although the fact that we have had the one disease without the other tells strongly against the supposition, I admit, yet I cannot altogether divest myself of the idea that there is, nevertheless, some kind of connection between glanders and dysentery. In the old world the cause of the one disease would appear to have existed side by side with the cause of the other disease. I deduce this from the circumstance that where dysentery has ceased, glanders has died out; and from the other circumstance that where the conditions favour an outbreak of the one, they lead to an outburst of the other, disease. In England glanders seems to have run nearly a parallel course with dysentery and to have become extinct, as an epidemic, about the same time. Elsewhere in Europe both diseases remain, and they occur in the epidemic form almost simultaneously. Although not prepared with any reasonable explanation of all this, it yet appears to involve something more than mere accident, or coincidence.

96. Passing over other obscure equine diseases for which a cause may be detected possibly in mildews, I will briefly advert to the fact so well known to all bird fanciers and fowl breeders: viz., that cages and fowl-yards require to be kept sweet and clean, or the birds infallibly become diseased in a very short time. The least delay in attending to this matter is certain to be followed by an epidemic of some kind. This points either to some injurious gaseous product from decomposition, or to fungoid vegetation; and of the two things the latter appears the most probable. While upon this subject of fowls, I cannot refrain from alluding to a most interesting communication upon the disease which attacked the hens in the Crimea in the autumn during the siege of Sebastopol. When going through that valuable record of contemporary medical history, *The Lancet*, in order to glean all I could about the diseases of the Allied Armies, I fell in with a letter from Mr. G. E. Blenkins, an army-surgeon, in which he states that the fowls imported into the Crimea suffered from symptoms resembling those of dysentery. They had a bloody flux; and, on dissecting the birds that died, Mr. Blenkins found analogous appearances in the intestinal canal to those observed in the colon of man. Mr. Blenkins also mentions another strange circumstance; namely that the flux affected and killed the recently imported fowls only—those which had been in the Crimea some time before this disease set in escaping entirely, and laying their eggs as usual. This skein is too tangled to unravel.

97. These rough notes touching the close connection between the dung of animals and their own special diseases and the remoter and more obscure relation of the dung of one animal to the diseases of another animal, are all I have to offer. They are but disjointed speculations in their present form, but as they may have a nucleus, or nucleolus, round which thought may gather, I let them go for what they are worth. I will merely add that, however incapable I may be of demonstrating it with present materials, I hold the view that, whenever animals, domestic or otherwise, are subject to artificial restraints, or are accidentally placed in positions in which they are prevented from disposing of their excreta in the natural way, and the excreta are allowed to accumulate so that the animals are exposed to their influence; they will be liable to definite specific diseases in consequence—the specific nature of the malady depending, probably, upon the local and seasonal conditions which determine the character of the fungoid growths upon the excreta. Yet this deduction is not in accordance with the deductions arrived at by those engaged in the investigation of the plagues and murrains of cattle in modern days. But I go further in this direction. I do not stop at the proposition that the dung of an animal will cause specific disease in the organism only from which it comes. I believe that, although this may be the most direct and primary effect, and the most easily traceable; yet that there are other perhaps more remote effects and less evident at present, produced by the dung of one animal upon the organisms of other

animals. This is a more complex and intricate problem, one of high import and serious moment to civilised nations; but, in the present state of knowledge, it can only be suggested as a possibility.

98. This diversion from the more immediate subject of the dysentery germ may seem to some beside the purpose. And perhaps if I had the one naked object of establishing the correctness of the theory of the causation of dysentery, I need not have cumbered it with these collateral details. But I take a larger view, and I see that if I have apprehended the true cause of dysentery, a flood of light must necessarily be thrown upon the causation of the various diseases of man and animals that have been hitherto, or may be hereafter, touched upon. And where the general good is concerned, strict, rigid and sharply cut lines of demarcation between subjects, are not demanded. It matters not to me whether a man be saved from dysentery, or from enteric fever. I consider too that questions affecting the safety of the flocks and herds of this country are of great national concern. But independently of these aspects of the mode in which I have dealt with the subject, I would observe that the matters mooted, which may appear at first sight somewhat irrelevant, are not so very wide of the mark I aim at specially. There are direct arguments and arguments by the inferential process. It is sometimes the shortest road to go a long way round.

99. To return to the dysentery germ. That germ, as before remarked, is at present hypothetical. It has not been demonstrated and its existence has been seriously questioned. It has a very doubtful position in the scientific world, and I fancy I detect here and there some sly and covert sneers in the allusions of philosophers to this unrecognised parasite, or rather to its fellow parasites of other diseases; for the dysentery germ is a more recent discovery, and has not yet stood the fire of criticism on its own account as an independent germ. What has been said to the disparagement of the other germs, however, may reasonably be assumed to extend to this one. It is probably viewed in the same light as the cholera germ and the typhus germ, which are on probation as it were—merely accepted on sufferance. They are used as stop-gaps for holes in theories of causation, and most writers resort to them when hard pushed. Very few take to them kindly, or cordially, or genuinely, as though they had a thorough faith and belief in them. There is always a hesitancy, a qualification, an if. I do not refer to these evidences of reluctance to adopt the parasitic theory of disease with any intention to imply that the learned are wrong in thus delaying to accept views that have been acquired by the aid of the microscope, until more conclusive testimony is forthcoming than that which is dependent on the eye and brain of the microscopist. Great caution is to be expected, and indeed demanded, before men in certain positions yield their judgments to hypotheses involving large interests and questions of life and death on a grand scale. They must have

something more tangible than a barren *Bacterian*, or any *micrococcus*, which, though cultivated, has not borne fruit. If the German microscopists had established one clear indubitable fact; if they had proved either that a given germ would produce a given disease, or that a given germ came from a given source, and had then connected that source with a given disease; the whole matter would have stood upon a very different footing. But in the absence of such material proof in any one case from the German school, it would perhaps be unwise in some men to rely solely on induction and to accept the proposition that every zymotic disease has its special cryptogamic germ—especially seeing that the proposition has been challenged by other microscopists, and that the whole subject is yet in the realm of controversy.

100. As regards the causation of dysentery I do not require, as I have said, a germ of any kind to establish the correctness of my theory. It will admit of proof, or disproof, by a much coarser and more solid test than can be found in the microscope. Cryptogam or no cryptogam, will fœcal matter, under the conditions specified, cause dysentery or not? That is the simple issue I submit for trial, and, as I shall put it, it will resolve itself into questions of fact, of experimental essay, and of future experience. It is altogether independent of the microscope. Rough, strong, practical common sense, combined with opportunities for observing the origin of the disease itself, are all that is required to settle the pretensions of my theory. It involves no ingeniously complicated, refinedly elaborate, or obscurely profound dogmas. It wants no subtlety of intellect, or high perceptive faculty; and, indeed, it may be "apprehended of the people."

101. The dysentery germ, therefore, is an extraneous question so far as the simple and obvious cause of dysentery is concerned—a question with which I need not have hampered myself at all. It is an excrescence upon my theory—a theoretical *micrococcus*—which I might conveniently, and with more ease to myself, have swept off and put aside. But though it was a formidable looking thing to one who knows nothing whatever of the microscope to enter upon a large enquiry hinging principally on microscopical points; yet it seemed not beyond the reach of abstract reasoning to arrive at some sort of conclusion touching this much debated and long considered cryptogamic theory of disease. Besides, I considered I held the vantage-ground of many, by reason of the insight I had gained into the cause of dysentery. With this light the want of all special, or technical, knowledge, might not leave me in impenetrable darkness.

102. After balancing the evidence for and against the germ theory—that is the evidence hitherto given on both sides, so far as I am in possession of it—I am forced to the conclusion that it is overwhelmingly in favour of the specific germ. In fact as a matter of pure induction, I take it nothing can well be more clear. To me the argument is as sound as though it had been clinched by absolute proof. I accept it as it stands, without waiting for the

demonstration—which cannot now be far off. Indeed I cannot see how any mind accustomed to weigh such questions can well resist the deduction from the premisses. And I can only account for the delay in the general recognition of the germ theory, on the supposition that enquirers have not settled themselves down to the work of examining it thoroughly, earnestly and exhaustively. I do not say that the microscopists are right in individual instances. It is not improbable that they may have got hold of the wrong germ here and there; and subsequent discoveries will very likely cause considerable modifications in their details. Their subordinate views, or minor theories, or speculations, also, may be wide of the mark. They may be wrong as to the precise mode in which the parasite multiplies itself; or as to the particular organ, or organs, it attacks; or as to those that are employed in its elimination; or as to its effects upon the blood; or as to the way in which it affects the nervous system. Upon all these points and others they may be partly, or entirely, wrong. They may have been tempted into ingenious, but unsound, speculation occasionally, and have been tripped up remorselessly; and their suggestions as to the sources of the parasites may have been at fault, and they have been treated with derision. But what of all this? The great principle is untouched—the main point is not affected in the least. Granted that there have been errors of omission and commission in the directions indicated; the one really important theory still stands. Discarding the non-essentials with which the question has been surrounded, and disregarding altogether the false issues that have been raised, I fix on what I take to be the essential things to determine first; viz.—is this class of diseases produced by a specific germ? and, if so, is it a cryptogamic germ? Its mode of producing its effects is an after consideration.

103. That there is a zymotic germ, or substance of some kind, there is very little doubt anywhere. The kind of germ then is to be arrived at. It must come either from the inorganic, or from the organic, world. Gaseous products, mineral substances, and chemical compounds of all kinds, do not require a moment's thought, and may be put out of court at once. We are driven then to the organic world; and here there are virtually only three efficient hypothetical modes by which the specific germ can have place; viz. (1) by animal parasites, (2) by particles of matter derived from an infected source, and (3) by vegetable parasites. (1) As regards animal parasites. The nature of such diseases as cholera and dysentery, as shown in the suddenness of attack and the rapidity of their course, would exclude them, independently of the manner in which these maladies are propagated, and of the fact that animal organisms have not been traced in the body;—always excepting Dr. Dyes's unique discovery of a cause of dysentery in the viscous pellicle of plums; which, however, does not disturb the conclusion materially. Low animal organisms may, therefore, be set aside as an efficient causation of the specific germ. (2) These particles are no doubt efficient germs in some

exanthems, but it has been shown clearly enough that the principle of *contagium vivum* is not the active one in these diseases. Besides even if it were, it is merely a shifting of the point at issue, and confounding the question of propagation with that of causation. The origin of the disease, even though the affection be contagious, might still be either in animal, or in vegetable, parasites. (3) The factors of the germ, then, are, so far as we know at present, to be sought for in the vegetable kingdom.

104. The principal arguments for the fungoid source of the germ are these, and I do not find anything of sufficient weight to upset them. (*a*) There is no other efficient cause for the germ and it is the only one left by exhaustion—a very cogent, though, in natural laws, not a complete, argument. (*b*) Sporules, vegetable cellular growths, and the tissues of various parts of many cryptogamic plants, have been found in different portions of the human body by several independent microscopical observers for years past. (*c*) Not only have portions of cryptogams been found, but, in many instances, the spores, or sporules, have been cultivated and the fungus to which they belong has been produced and grown. It now, perhaps, becomes a question as to the amount of trust, or confidence, to be placed in the microscopist. We have to determine for ourselves how far we may rely on the accuracy and on the fidelity of the observer. Has he deceived himself, or is he wilfully deceiving us? In matters of human testimony such contingencies are, of course, always possible. When, however, it is remembered that every microscopist who has attained to any position in Europe, is checked by the certainty that his published observations will be critically examined, and that his experiments will be repeated, sooner or later, there is at once a material guarantee that he will take due precautions. He is not likely to risk his reputation by rash statements, or by describing illusory, or imaginary, objects, which nobody else is likely to find, and which he himself cannot show to others. Enthusiasm, and its ally, rashness, may now and then induce a philosopher to jump to conclusions on insufficient data; but a habit of this kind is apt to be rather sharply punished by the world. So much for mere errors of vision, or of judgment. If we now consider the question of wilful misrepresentation, or deliberate fabrication, it will be at once seen, I think, that the chances of this are very slight indeed. I fail to perceive any adequate motive for inventing a cryptogam. There is nothing that I see to be gained by a microscopical fraud. Forging a germ is a most unlikely scientific crime. It might be done as a piece of elaborate waggery, but it seems highly improbable. When mistakes are made by microscopists, or when descriptions of things are given by them which cannot be verified by others, or be demonstrated by them to others, we may conclude, reasonably I think, that there has been some unintentional error somewhere. And when we find several microscopists in different countries concurring as to things seen, I conceive their statements may be accepted as facts, without hesitation. It may be regarded I submit, therefore,

as a certainty that fungoid growths of some sort are found in the human body. It is more than 20 years ago since the fungoid theory of cholera was started, and since then scores, or hundreds, of observers have found portions of cryptogamic plants in the matters vomited by, and in the dejections of, cholera patients. I do not know to whom belongs the merit of the discovery, but my impression is that it was made in England. Mr. Swayne of Bristol read a paper on the subject, I believe, shortly after the cholera invasion of 1849; but whether he originated the view or not, I am unable to say. The idea, however, spread to Germany where it took stronger hold than it appears to have done in England. The German philosophers nursed it until it expanded into the germ theory of disease. I do not know precisely, but I imagine Hallier was the first to propound the theory, as his name seems to be invariably associated with it. At all events he has described many new fungi which he has found and which he has called (rightly or wrongly remains to be seen) the specific germs of certain diseases. And the point now is, not whether the fungoid growths he describes are the true germs of the disease, but whether the descriptions themselves can be implicity relied upon. Judging Professor Hallier by the estimation in which he is held by his countrymen, and others, and reasoning on other extrinsic grounds, I have no hesitation in saying that I accept every statement as to matters of fact made by this observer as literally and absolutely correct to the best of his knowledge. When he says he has discovered a germ, I believe he has discovered it, or is under the impression he has discovered it. If he tells me he has discovered the dysentery germ, for instance, the only question on my mind is whether the fungus is the right one, and not whether he has discovered a fungus. That he has found what he describes, I take without qualification.

105 (*d.*) The International Cholera Conference at the meeting at Constantinople in 1866, adopted the germ theory as regards cholera; and though their decision may not be regarded as a final settlement of the question, it must necessarily have its due weight with all thinking men. When delegates from different nations agree to a proposition of this kind, it argues that the evidence must have been very conclusive indeed. Occupying as they did an official position of grave responsibility, they were hardly likely to stultify themselves by rashly declaring their adherence to a particular side in a *quæstio vexata* of such magnitude and importance. The fact of their having done so implies care, study, and deliberation. For they would naturally have the fear of the derision of the civilised world before their eyes, in the event of their committing themselves to a hypothetical blunder in solemn conclave. So although there will always be, of course, unbelievers who decline to subscribe to articles of pathological faith drawn up by a medical convocation—no matter how orthodox—still the generality of men must admit that this decided step taken by the Conference is a strong argument in favour of the germ theory.

106. (*e.*) In the Quarterly Journal of Microscopical Science for 1871 at p. 197 there is a very remarkable passage, headed "*Fungus as a cause of Whooping-cough,*" in which it is stated that Dr. Letzerich "thinks he has discovered" the fungoid growth which produces this disease. "The expectorated mucus in patients is said "to contain masses of brownish-red spores with occasional threads "of mycelium." These were cultivated on bread soaked in milk, and "masses of the fungus thus obtained" were introduced "into "the trachea of young rabbits." These rabbits soon got "a "cough of a very violent and noisy character; in fact, a genuine "whooping-cough." These rabbits on being killed were "found "to contain an enormous quantity of the same fungus as that met "with in the sputa from human whooping-cough; and, in fact, "the mucus expectorated by the rabbits showed precisely the same "appearance. Dr. Letzerich had already published very similar "observations on a supposed fungus causing diphtheria; but "neither set of observations seems, as yet, to have been confirmed "by any other investigator."

An announcement of this kind is painful and humiliating. Who would suppose that thousands of lives may have been lost that might have been saved by the confirmation of the last set of experiments alluded to? Is it not deeply humiliating that such matters as these, involving questions as to the origin of such a deadly pest as dipththeria, should be left to the chance attention of some philanthropic investigator? No doubt the man will be forthcoming sooner or later who shall devote himself to the subject, and shall successfully demonstrate, not only the fungus of diphtheria—if there be one—but its source. But why could not enlightened nations appoint some permanent body of thoroughly competent men for the purpose of testing all such observations as have been made by Dr. Letzerich? What valuable time might be saved! And what false pretensions might be at once exposed! As the matter now stands with regard to both whooping-cough and diphtheria, who is to say that the deliberate statements made by this physician are without foundation? And how long will it be before the set of experiments with the rabbits will be repeated? This set, at all events, might be tried without much difficulty or delay. And although the determination of the question of the fungoid origin of whooping-cough in the affirmative, might not have such immense and immediate practical importance as the similar mode of settling the fungoid origin of diphtheria; yet relatively it is just as material and important to establish the fungoid germ in one case as the other. Besides if Dr. Letzerich's statements as to whooping-cough be confirmed, it goes a long way towards the acceptation of his discovery as regards diphtheria.

To the present consideration, the fungus of whooping-cough is more important than that of diphtheria. In the latter case Dr. Letzerich's fungus stands on the same footing with Hallier's fungi;—it is a *fungus unattached.* But the other fungus is a very different thing. Since the discovery of the "ague-plant" by Dr.

Salisbury, this claim of Dr. Letzerich's is the first of its kind. [I was not aware of it when I wrote about the American fungus.] What strikes me as so odd in connection with both these discoveries is that the pathologists of Europe do not appear to apprehend their full significance, as regards the fungoid origin of cholera and other diseases. It seems altogether to have escaped notice how much these two separate sets of observations carry with them—if correct—and how urgent and pressing is the necessity, therefore, of dealing with them and determining upon them one way or the other. Dr. Salisbury's observations are the completer; for he has not only produced the germ, but he has connected it with its source. It would, however, entail some inconvenience to scientific men in Europe to visit the valleys of the Ohio for the purpose of testing his experiments on the spot. But the rabbit test is so easy of accomplishment anywhere, that it is strange it should not have been tried in all directions immediately Dr. Letzerich's account was published.

In dealing with a matter of this kind in this part of the world one is necessarily liable to be at fault. It is so hard to learn what has been done. In calculating the chances as to the correctness, or otherwise, of these observations of Dr. Letzerich, there are so many sources of fallacy that one is left in great uncertainty. I will therefore content myself with saying here that I must accept these observations, until I learn that they have been proved to have been incorrect. There are some distinct, circumstantial, statements as to matters of fact, of a very peculiar kind. I can only say that if other observers fail utterly in inducing this singular cough in rabbits, and if Dr. Letzerich himself is unable to reproduce the cough, as before, and to exhibit the rabbits in the condition he describes, to other persons; I should not envy him the position he would hold. But as I cannot suppose it possible that any man in his senses would have the folly and the hardihood, even if he had the will, to fabricate observations, the falsity of which could be so readily shown, I come to the conclusion that Dr. Letzerich's discovery is a real one, though he has failed in the great desideratum of connecting the germ with its source. It is therefore included in these arguments in favour of the germ theory.

107. (*f*) The production of measles by means of mildewed straw, before mentioned, is another strong argument. In fact this and the other great discovery (*g*) of Dr. Salisbury of the "ague-plant," are, of themselves, quite sufficient to establish the germ theory. Enough has been said previously to show the view I take of these discoveries and the large inferences I draw from them.

108. (*h*.) There are other reasons, less pronounced, why I give in my adhesion to the germ theory. But over and above all that has been published hitherto, I have the knowledge, as I believe, as to the cause of dysentery. And this supplies me with such cogent additional reasons, that I consider the final establishment of the fungoid origin of disease is not far off.

109. If now one considers the arguments that have been advanced against the germ view, we shall find that they amount substantially to these;—that some observers have not succeeded in finding the spores, or sporules, or any vegetable growths, answering to the description given by other observers; that the inability of Hallier and others to demonstrate the matrix, or nidus, or original substratum of the germs, throw doubt upon them; that the connection between the so-called germ and the disease has never been sufficiently shown—the two things not having been clearly exhibited as cause and effect; that the *modus operandi* of these cryptogams, as explained by various pathologists, is so vague, fanciful and contradictory, and so subversive of other laws, that they suffice to upset the theory as to the germ; and that in the present state of our knowledge so much dependence should not be placed on microscopical observations. This seems to be about the pith of the objections raised, and I think there can be no necessity, after what has been said, to deal with them formally. The only point worth noticing is the negative one that particular fungi described by some, cannot be found by other, observers. But there are so many ways by which a microscopist may miss a particular thing he searches for, that the omission cannot stand a moment against the positive statements of other microscopists that they have found the thing. When the American observers say they cannot make out the *Coniothecium* described by Hallier, and thence conclude that it is an apochryphal fungus, and moreover go so far as to deny the germ theory of disease, it seems to me that they draw rather large conclusions from very small premisses. And how about the "ague-plant" of Dr. Salisbury? Do they ignore that fungoid germ? Do they throw Dr. Salisbury over? How little honour have prophets in their own country! But Dr. Salisbury may be a rank impostor. He may have coined the palmelloid fungus and the whole series of observations upon intermittent fever. Either this, or he has established the germ theory beyond question by a brilliant idea most ingeniously and beautifully worked out. Which is it?

110. Whence comes the dysentery germ? As far as my view is worth anything, it is that this germ is a vegetable germ, and that it comes from a mildew formed on human fœces under certain conditions; but whether it is a *Hydrophora*, or a *Mucor*, or a *Pilobolus*, or any known or unknown mildew, I am not, of course, able to say. I am under the impression that Hallier is correct in claiming the discovery of the germ. I have no doubt at the present moment that the sporules he has cultivated, and the fungus he has grown thereby, may be traced to the source here indicated. If so—if this consummation be achieved—it will be a glorious triumph for the microscopist, and it will set the question of the fungoid origin of disease at rest for ever.

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111. Without wishing to entangle myself in abstruse physiological and pathological problems, involving questions of minute

anatomy, organic chemistry, and other things of which I am ignorant, I must yet touch gently upon the modes, or presumed modes, by which these vegetable germs produce their constitutional effects and special lesions in the body. Or rather I shall confine myself chiefly to the manner in which the dysentery germ produces its specific results. I enter upon this part of my subject with some reluctance, I admit; but without wading out of depth I may perhaps suggest one or two views for others to work out. I can see their practical bearing, though I am incapable of handling them.

112. Does the dysentery germ act by inducing chemical, or molecular, changes in the blood? Or is its action upon the glands of the large intestine purely mechanical? The point is not interesting merely, or remotely important, but its determination would lead straight to material and immediately available conclusions respecting the causation of some other diseases. From the whole of the surroundings of dysentery, I arrive at the conclusion that the local mischief in the glands is produced mechanically in some way, and is not caused by chemical or vital action. The mode in which the disease attacks healthy persons, the nature of the symptoms and the character of the malady would seem almost to preclude the idea of blood poisoning, in the sense in which it is usually understood. If then the change produced in the glandular structures of the intestine are due to mechanical causes, the next thing to determine is whether they are extrinsic, or intrinsic, to the circulation. It is possible that the mucous membrane of the alimentary canal may be a sufficiently extensive, and favourable, substratum, for the development of myriads of the fungoid growths which cause dysentery. And it is possible to conceive that these fungi may pass along the canal until they are brought into relation with the glands; and further that the glands may have some special, or elective, affinity for them, by which they may be excited, or impelled, to attract them; when from some cause they produce irritation of these glandular structures, and thus set up the diseased conditions which end in hœmorrhage, ulceration, and perhaps sloughing of the whole coat of the intestine.

113. The minute morbid anatomy of dysentery is not so clearly made out as that of many diseases. Professor Maclean mentions that Dr. Parkes has shown that, "at the very commencement of the morbid action," "the solitary glands are enlarged in various degrees." "They are seen to be distended with a white exudation, "some with a dark central spot, and all surrounded by a vascular "ring." "Dr. Parkes was led from his dissections to the belief "that ulceration almost always begins in the glands themselves, "very rarely around them, and only occasionally, in very rapid "cases, by effusion of fluid beneath the mucous membrane." All this would seem to point clearly enough to the solitary glands of the large intestine, as the site of the commencement of the morbid action. If so, the question arises whether there is any essential anatomical difference between the solitary glands in the large and

small intestines, either in their structure, or their blood supply. Because it is somewhat difficult to understand how glands which are common to the whole intestinal canal, should be liable to become diseased in one part of it, unless there be some structural and therefore functional variation in them, not hitherto described, or suspected. If they were identical throughout, one would expect the same lesions in them throughout; and therefore the selection of the solitary glands of the colon would seem to argue some peculiarity. The point is raised here, because it bears upon the question whether the local manifestation commences on the villous surface of the intestine, or originates in the net-work of capillaries supplying the glands.

114. If the observations of pathologists and microscopists, however, are to be thoroughly depended upon, it would certainly seem that the only rational way of explaining the local effects of the dysentery germ, would be by assuming that it arrives at the large intestine by way of the blood, rather than along the alimentary canal. The disease probably follows the same course with others of the same type: and by analogy we may assume, that if the "ague-plant" is found in considerable quantities in the urine, as Dr. Salisbury affirms; and if whooping-cough can be induced in rabbits, as Dr. Letzerich has described; and if fungoid growths of many kinds have been found in the blood, as many microscopists have shown; the topical disturbances of dysentery are set going by its germs first getting into the circulation, and by being then conveyed to the bowel through that channel. The law applies, in fact, to so many diseases, that, in the absence of any special reason for exempting this one, it may safely be taken to come within its operation.

115. And now comes the really difficult part of the subject. Having got the hypothetical dysentery germs into the blood, how is it that these particular fungoid growths, and no others, should make their way to the colon and cause a bloody flux? And why do other germs invade the small intestine causing enteric fever? Or the air-passages producing diphtheria? The difficulty of accounting for the selections of points of attack, made by the various germs, would be equally great, either on the assumption that their mode of action is a vital one, or on the hypothesis that it is a mechanical one. As regards dysentery, however, as has been said, the history of the disease favours the view of mechanical agency. And if this can be shown, it will govern some other cases, and probably be of most material assistance towards elucidating what is now obscure with reference to the mode of action of some germs, and possibly end in the discovery of others.

116. Before proceeding to discuss the special subject of the dysentery germ, I would observe that the well-known fact that all the exanthems, and the zymotic diseases generally, have a tendency to affect mucous membrane, or glandular organs connected with it—that is viewing the skin as an extension of mucous membrane—has led to the inference that this determination to the

superficies is connected with the excretion, or elimination, of the morbid material. Still this does not explain why every form of fungoid growth should have its own peculiar mode of exit, accompanied by its own special train of local lesions and constitutional effects. There is another trait that these diseases are all observed to have in common; and that is that whatever surface may be selected as the field of departure from the body, or as the region of attack, the morbid results are invariably associated with the extravasation, or the exudation, of blood, or of some of its serous, or plastic, constituents. The kind of material thrown out and the nature and position of the tissues into which it finds its way, together with its amounts, seem to be the things which give distinctive character to the disease. The degree of symptomatic fever, or constitutional disturbance, is regulated entirely, as it seems, by the extent of surface implicated, by the special character of the action set up, and by the greater or less importance of the functions interfered with. The modes by which fatal results accrue in affections of this type illustrate this. The end, and perhaps the aim, of the expulsion of the blood, or of some of its elements, from its vessels is supposed to lead to suppurations, of various kinds, and to ulcerations of the tissues, by means of which an open surface is established and an outlet more readily afforded to extraneous matters.

117. There is a similarity between dysentery and diphtheria in the results of the morbid action set up in the two diseases. Niemeyer says that "the anatomical changes found in the intestines "of dysentery patients, on autopsy, are a type of diphtheritic "inflammation. The diseased portions of mucous membrane are "infiltrated with a fibrinous exudation." This membrane in some instances is "to some extent infiltrated with a grayish-white, soft exudation, covering the epithelial coating." Niemeyer quotes Rokitansky to the effect that "in the highest grades of dysentery," portions of the lining membrane of the bowel are "not unfrequently thrown off and passed as tubular pieces." The analogy therefore is by no means forced. *Mutatis mutandis* the one disease is a representation of the other. Does the similarity of effect suggest a similarity of cause?

118. On the hypothesis that the fungoid germs cause the particular local disorganisations characteristic of the disease, in consequence of the efforts made by the system to throw them out of the circulation, it is advisable to consider the probable, or possible, modes, by which the dysentery germ may cause its special lesions during the process of expulsion. The difficulty, if not impossibility, of getting at any sound conclusions on such a point, at this stage of our knowledge, is clearly present to my mind. I shall not, therefore, fritter away much time in speculations that may be soon exploded by actual information upon the matters discussed. Still I may risk a few thoughts.

119. There are several ways by which it may be supposed the dysentery germs are arrested in, or held by, the glandular struc-

tures of the colon, pending their eventual extrusion by hœmorrhage, by suppuration, or by ulceration. (*a*) The diameter of the capillaries may be so small as to delay, or stop, the germs. (*b*) The anastomosis of these minute vessels may be so complex, or intricate, or tortuous, that these foreign substances cannot pass freely and so finally, by stasis, bar the passage. (*c*) This result may also depend on the peculiarity of the structure of certain portions of the cryptogam, by which they are caught in the loops of the local capillary system, after having passed through the other portions of the body with the current. This is a matter of measurement, perhaps, and also of the roughness, or smoothness, of the spores, or of such parts of the fungus as may find their way into the vessels. Some of the membranous portions of some mildews—that of the membrane of the periodole of *Mucor mucedo*, for instance—are described as being clothed, or studded, with spinous-like processes. The spores of some genera and species of mildews are also, of course, larger than others, and their sporanges have various modes of letting the spores loose. The question as to the particular portion of mucous membrane a special fungoid growth shall affect, may, therefore, all turn upon the configuration of the plant and the nature of the capillary vessels. So little is known of the mildews growing on excreta—more especially on human excreta—that it would be futile to speculate on the probable causes of the arrest of the dysentery germs in the large intestine. There must, however, be some prominent and marked reason for this—a reason so pronounced that there should be no great difficulty in arriving at it. (*d*) In addition to these purely mechanical obstacles to the free passage of the germs, something is also due, probably, to that inherent power possessed by organic structures, through which they seem to exercise a quasi choice, or selection, in fulfilling their functions, or in carrying out their purpose. The description of vital force I mean appears to be connected with some irritability, or excitability, or susceptibility, to external things, or conditions, by which tissues make an election in dealing with either ordinary, or extraordinary, circumstances. This elective affinity, as it has been aptly named, is not unlikely to have its influence in determining the dysentery germ to the mucous membrane of the large intestine, and all the other germs to their special localities. At the same time I strongly suspect that this elective affinity is itself controlled and influenced, to a certain extent, by mechanical conditions. It may be that the seat of the dysenteric affection is the best, after all, that the body has for the elimination of increasing and multiplying fungoid growths of this particular description. In speaking of the physical, or mechanical, obstructions that might be caused by the sporules, sporanges, periodoles and columellæ of the dysentery mildew, it will be borne in mind that the agency of these portions of the hypothetical fungus can be supposed only when the disease has been caused by air-poisoning. These parts of the reproductive organs of a mildew cannot of course get into the body by water-pollution. And this essential difference in the configuration of air and water germs, if the difference suggested be a fact, will afford, perhaps, an explanation of the efficacy of the change of air to dysentery patients, and will also serve to explain how it is that "bad-water" cases are never so formidable as those occurring by way of the atmosphere.

[B.]—TEMPERATURE.

120. The second proposition starts with saying *that a certain degree of temperature* is a necessary condition for the evolution of the dysentery poison from fœcal matter. The question as to the degree need not take up much time. As a general rule it would appear that the poison, whatever it may be, is developed more rapidly, or with greater facility, in low latitudes. Heat certainly seems to favour the formation of the active agent of dysentery, or causes it to mix with the air more readily. At the same time it is very clear that it can be formed and can be disseminated in very cold weather, as was found in the Crimean winter season. The probabilities are—though I speak without precise knowledge—that the dysentery poison may exist and be potent in any temperature short of the freezing point. Congelation no doubt arrests both its production, and its spread by atmospheric agency; though ice from "bad water" may possibly contain it. It has been found by experience that those periods of the year when the days are hot and the nights cool, are most frequently marked by outbreaks of dysentery:—though the fact may perhaps admit of another explanation besides that of mere alternations of temperature.

[C.]—ATMOSPHERE.

121. The next portion of the second proposition states *that a certain amount of exposure to the influence of the atmosphere* is a necessary condition for the evolution of the poison. And this is the great essential condition of its formation—the special requirement for its production. It is this peculiarity which happily brings dysentery completely under human control:—so completely indeed that nations may rid themselves of the epidemic pestilence at will, and armies of occupation may entirely escape its ravages, except in certain tropical regions where the habits of the people preclude all hope of safety. In every country in the world, however, where the air is not contaminated, and the water supply is not polluted, by the resident population, armies may in the future conduct many of their operations without the slightest fear of being decimated by this hitherto invariable scourge. I say it unhesitatingly, advisedly and without any qualification whatsoever, that, had the simple fact now made known, been known at the time of the Russian war, and been acted on, not one man in the Crimea need have died of dysentery.

122. In venturing into unknown regions and in treading upon dangerous ground, I have hitherto been guarded and careful. But here I feel myself justified in assuming a firmer position and in proceeding more boldly. I have no doubts and have to make no reservations, now. I have thoroughly satisfied myself of the

correctness of the propositions advanced, by what I consider sound induction from a large array of facts. Therefore I do not shrink from the consequences that may be involved in stating such large and important conclusions. And I affirm that nations may extinguish epidemic dysentery, as England has done, by the observance of the simplest laws; and that their forces in time of war need but rarely engender the disease, if only they will consent to the adoption and employment of practical and readily contrived expedients;—expedients which will suggest themselves at once, when it is comprehended that, if human excrement is not exposed to the air, there can be no dysentery.

123. If this be not proved to demonstration—so far at least as words can demonstrate a thing which must after all be left to the actual proof—I have erred most egregiously in my reasoning; and I shall perhaps deserve the punishment which, I am perfectly well aware, the highest motives would not suffice to avert. I send this forth with my eyes open. I know the kind of treatment it may meet with. I have seen how a noble discovery emanating from this colony, where it has already been shown to be eminently successful by rescuing several persons from the very jaws of death, has been jeered at and derided as being neither true nor new. And if the man whose soul had the courage to put into execution the heroic remedy his brain suggested, and who thus demonstrated the correctness of his deductions by absolute and irrefragable proof;—if this man, with actual success attending on his efforts, has not only not been warmly hailed in other parts of the world, but has been received coldly and sneeringly, and as though his bold conception to save human life when all other means have been seen to fail, was a thing to be deprecated;—I say, if this man has been so ungraciously met, how am I to come off, in presenting a bald theory for which at this moment I have nothing to advance but induction? I have not a solitary experiment to offer in support of my views—not even a confirmatory germ. If I be shown to be correct, perhaps the chastisement may not be too severe; but if I turn out wrong in my reasoning nothing can save me. Yet, right or wrong, I shall take my chance of any personal annoyance, or chagrin. I shall risk fearlessly and trust to having fibre robust enough to stand the strain, whatever comes.

124. The amount of exposure to the air necessary before fœcal matter can form the dysentery poison, is a question involving considerations of time and extent of surface exposed. There is clearly a space of time during which excrement is incapable of giving off the poison; and a greater or less superficial area of excrement must also be left uncovered and open to the action of the atmosphere, or the poison will not be formed.

125. As regards time, there are no means of determining the shortest period at which fœces will form the poison, nor the longest period of exposure they will bear without losing the power of forming it. They are both material practical points to decide,

obviously, but at present one can only offer general observations. The endemic outbreak of dysentery among the Dutch troops at the Cape [17], shows that the poison may at all events be engendered within a month. The probabilities are that with favouring seasonal influences, it may be produced considerably under that period; but it would be premature to fix minimum limits. Then with respect to the length of time that fœcal matter retains the faculty of forming dysentery poison, information is still more vague. Of course there must be a period of disintegration when the elements in excrement can no longer furnish the necessary conditions for the origination, or maintenance, of the poison. This period may be hastened, or delayed, by external circumstances, in every individual case; so that it would be hazardous to say that the conditions will be broken up in so many months, or even years. But I think it may be safely said that when excrement lying on the surface of the soil, has so far lost its characteristic qualities as to be hardly distinguishable from the earth on which it lies, it is then incapable of giving rise to dysentery;—and not until then is it certain to have been deprived of its poisonous properties. Even when it has been desiccated on its exterior, so as to be devoid of odour and the outward semblance of fœcal matter, yet there may be some portions in the interior of the mass that may have been conserved, and may be still potent, should wet weather supervene to wash off the outer crust. How long the collections of fœcal matter accumulated in privies will preserve their competency to produce the poison, when removed from their receptacles and placed under other conditions, it is difficult to form any opinion. It would seem clear, however, that, under ordinary circumstances, night soil is always capable of giving rise to dysentery when spread on the ground at moist or rainy seasons, supposing it to be unmixed with other material and that no precautions are taken to prevent the formation of the poison. Whilst fœcal matter, in fact, is preserved as it is in privies, middens, or cess-pits, it may at any time be, and not unfrequently is, converted artificially into a material which shall cause endemic dysentery, when other causes combine. Kept together *en masse*, as voided, human ordure cannot, as I believe, give rise to the flux. Whether this be from the presence of the salts in the fluid portion of the contents of the receiving vessel; or from the small extent of surface exposed to the air, I am not in a position to say. But seeing that many privies in poor neighbourhoods, where dysentery is not met with, are so constructed that the fluids drain away rapidly and the solid excreta only remain, it would seem to be partly, if not mainly, due to the limited proportional surface exposed; though at the same time it is not improbable but the ammoniacal nature of the liquid has some effect in arresting, or preventing, the formation and evolution of the dysentery poison.

126. The maximum and minimum periods of time during which fœcal matter can create dysentery are indeterminate at present, and must remain so until larger experience is brought to bear.

It is, at all events, quite clear that the length of time required is not so great as to exclude the Laplanders and Greenlanders from experiencing an epidemic during their short and nightless summer, supposing the conditions to be there. I have not had time to search through the Arctic records for evidence as to this; but it would not surprise me in the least to find that every habitable country in the globe has, at some period or other, furnished isolated instances of dysentery, or has even been subject to epidemics.

127. The next question is as to the extent of surface required to be exposed so as to ensure the generation of the dysentery poison. And here again one cannot reduce the matter to anything like certainty. It is manifestly impossible to say that a given amount of excrement must present so much area in proportion to bulk to the effects of the atmosphere, or the dysentery poison will not be evolved. Yet although this cannot be determined with precision, I submit there is nevertheless a direct relation and an exact one, though variable in different latitudes, between the production of the poison and the superficies of the excrement left open to atmospherical influences. I cannot formulate the law for want of data; but that there is a law there can be no manner of doubt. The only thing that can be said is that a certain amount of surface exposure is a necessary condition; but what that amount is, can only be guessed at, or arrived at approximately, at present. It will be a practical point to be determined hereafter, by those who have the hygienic management of troops, or the sanitary arrangements of cities with mixed populations, in warm latitudes, under their control.

128. By way of throwing some light on the matter, I will detail the views that occur to me. The most common mode by which the dysentery poison is formed, whether it results in causing only a single case or two, or an epidemical outburst, is by leaving fœcal matter on the surface of the soil, just as it is passed in the act of defecation. By this means the atmospheric conditions necessary to the development of the poison have the most perfect play. There is room for the action of the air over a larger extent of surface in proportion to the bulk than is found under all other ordinary circumstances. Assuming that the mass be left undisturbed as deposited, the factors are under the most favorable conditions for evolving a poison which shall be capable of transmission by the atmosphere and shall also have the power, as I believe, of polluting water. Whether the local depositions of human excrement do, or do not, lead to the formation and evolution of the poison, will depend upon the presence or absence of the other necessary conditions. All other things being equal, however, there is no other natural process so conducive to the formation and evolution of the poison. In fact the exposure of fœcal matter in this manner has been the great universal cause as well of isolated cases of dysentery, as of most great epidemics—except in China.

129. It follows that if the conditions referred to be the most favourable, anything which tends to interfere with those conditions

will also tend to interfere with the result. If, for instance, these deposits are covered, or partially covered, with earth, or dust, or if interfered with by the trampling of cattle, or by the larvæ of flies, or by such other means as will readily occur to residents in India and other tropical countries; or if thrown together for scavenging, or agricultural, or other, purposes; their power of furnishing the dysentery poison will be diminished in proportion to the degree of interference. All agents which lessen the surface of fœcal matter exposed to the atmosphere, lessen its capability to produce the poison, by the amount of surface lessened. Complete occlusion destroys the power altogether. So long as excrement is kept from contact with the external air, so long it is inert—as far as dysentery is concerned. The interstitial air in the earth's crust is insufficient to cause the evolution of the poison.

130. Hence it will be seen that all contrivances, even of the rudest description, by which communities have lessened the exposed superficies of their excreta, have answered the purpose of reducing the amount of dysentery among them, precisely as the contrivances have been more or less effective in the direction indicated. Strange to say some of the most cultivated and most refined of nations in modern days have shown an indifference to, or a disregard of, this particular subject, which has resulted in most deadly epidemics. Almost the whole of the European continent has tolerated the existence of habits and customs among the lower orders, even in the capitals, for which it has paid the severest penalties in the shape of disastrous outbreaks of dysentery and doubtless also in attacks of enteric fever. But these questions come more properly under the *Third Proposition.*

[D.]—MOISTURE.

The last condition given in the second proposition is that *a certain quantity of water, or moisture in the air,* is required for the evolution of dysentery poison from fœcal matter.

131. This is a highly necessary, and indeed an essential condition; for without rain, or a moist state of the atmosphere, the production of epidemic dysentery is an impossibility, no matter how much human excrement may be lying exposed on the ground. Dry conservancy of fœces stops the production of the dysentery poison as effectually as total deprivation of external air. The desiccation of the outer surface of ordure and the consequent hardening and final pulverising which take place, destroy for the time being all poisonous emanations. And if continued until the mass is disintegrated and reduced to dust, or so that none of the traces of fœcal matter are present, it is no longer capable, under any circumstances, of giving rise to dysentery. Whilst, however, any portion of the interior of the excrement coheres and retains its distinctive character, as has been said, it may be rendered efficient as a cause of dysentery when brought under the necessary conditions. As regards the quantity of moisture required, it would seem that all that is necessary is a slight humidity of the atmosphere—

just sufficient to prevent the rapid drying up of the external surface of fœces. The hygometric state of the air in many tropical countries is such that there is almost always present a sufficient amount of water to keep the exterior of excrement in a damp state. Where this obtains the conditions for dysentery are rarely, if ever, absent. In semi-tropical and temperate climates, the great outbreaks of the affection generally occur at those seasons of the year when the days are hot and the nights sufficiently cool to cause dews or fogs; or else when the rains set in. At these periods such human excreta as are lying on the surface of the earth are placed under the most favourable conditions to develope the poison.

132. In certain parts of the East and West Indies, Ceylon, Borneo, Java, and all the islands in the Polynesian group, besides many parts of South America—in fact wherever there is a hot steamy atmosphere—epidemic dysentery may occur at any time when the other conditions are present; and when the exposed excrement has not been seized upon, or usurped, by the factors of the specific poisons of other diseases—a most important principle in the law of epidemics. But in countries like this of Australia, a great portion of Europe, and a large slice of Africa, the climate is so dry for two thirds of the year that the poison of dysentery could not be developed on a large scale, under any circumstances, during that period. Taking the experience gained in this colony of Victoria, it was found as a matter of fact, that the miners on the gold-fields were free from dysentery from November 1851 to March 1852, during the whole of which time scarcely any rain fell. About the middle of March, although the drought continued, the nights became sensibly cool; and, the sky being always cloudless, dews began to fall. Before the end of that month every gold-field on which a few hundreds of miners were collected, from one side of the colony to the other—from Bendigo to the Ovens—was visited simultaneously with a clap of epidemic dysentery, such as has been rarely surpassed in any part of the world for extent and malignancy.

133. So long as the perfect dryness of Australia, a dryness which parches and scorches everything, lasted, the excreta of the thousands of miners, which had been deposited for the most part in the immediate vicinity of the gold workings, were perfectly harmless and innocuous. A few minutes of a hot wind sufficed to render all fœcal matter incapable of giving forth the poisonous emanations. But no sooner was this state of affairs changed, and the enormous surface accumulation of fœcal matter in the neighbourhood of the diggings was kept in a moist condition for several hours daily; or, where protected from the sun's rays, was never allowed to become dry or hard; than the factors of the dysentery poison were set to work with the most deadly rapidity and activity. The winter of 1852 was very wet and the epidemic raged throughout, ceasing almost suddenly when the warm weather at last appeared. There was then a complete lull during the summer of

1852 and 3. But in the autumn (March) when the former conditions were revived, there was another epidemic outbreak, which continued during the winter, but was neither so extensive, nor so malignant, as the original outbreak—though still terribly fatal.

134. Here then the occurrence and recurrence of epidemic dysentery was coincident with moisture in the air. Of course two instances are insufficient to establish a law, but if enquirers will be at the pains to look into the history of the disease, I venture to say they will find that all the epidemics of which we have any record have been associated with, or preceded by, rain, or damp weather. I have been unable to discover one instance to the contrary. The presence of moisture in the air, therefore, I take to be an essential condition, for the formation and evolution of the dysentery poison from fœcal matter. Whether it is necessary for the purpose of facilitating putrefactive fermentation, or some other form of change, or decomposition, in the excrementitious substance, I do not know—though it will doubtless be understood that my view is that the ultimate result is parasitical vegetation—the moisture being a precedent condition indispensable to the growth of the mildew of dysentery.

[E.] OTHER CONDITIONS.

135. Under this head may be grouped all those telluric, electrical, photo-chemical, cosmical, vital, and other forces of which we know so little. It is possible, if not probable, that the formation and evolution of the dysentery poison may be influenced and controlled by some one, or more, of these mysteriously working natural agencies. I can conceive that the actinic rays of the sun, for instance, may have a most material effect in modifying the nature of the poison. The electrical conditions of the earth, too, may affect the amount of poison produced at particular seasons. It is not unlikely that the ozonic, or antozonic, state of the air may check, or favour, the spread of the poison. There can be no reasonable doubt that many of these conditions are somehow involved in severe epidemics of all kinds; though the precise mode of action has not yet been made out. They are generally implied in speaking of *seasonal* conditions.

PROPOSITION III.

That the quantities, or values, of the poison formed are determined both by the amount of excrement submitted to the conditions, and by the more or less perfect application of, or submission to, the conditions.

136. This proposition, assuming excrement to be the base of the poison, is so obvious that it might, perhaps, seem to require but little remark. It is, however, the means by which the occurrence of idiopathic, or isolated, cases of dysentery, as well as of epidemics, is readily explained. The matter has been referred to by the authorities as being somewhat obscure and as requiring explanation; for the occasional incident of dysentery in London has not been satisfactorily accounted for. And, indeed, without the key it

would be extremely difficult, if not impossible, to arrive at a complete and satisfactory explanation of the phenomenon. With the key nothing is more simple than to find an efficient cause for these exceptional local cases of dysentery. The population of London includes men of all nations and of all habits. Some of these have singular modes of dealing with their excreta, and many probably leave them, designedly and wilfully, exposed upon the surface of the ground. Houseless wanderers are forced of necessity, perhaps, into the Parks and open spaces, or into the back yards of unoccupied houses. Without, however, citing the different ways in which excrement may be deposited upon the soil in London, by the eccentric, the lunatic, the dirty, and the necessitous, it will be evident that, in spite of all police and hygienic arrangements, it must happen that more or less fœcal matter will always be exposed in that city to the influences of the external air. It will be granted too, I should suppose, that with the utmost vigilance which sanatory inspectors, or commissioners, or executive officers of all kinds, could display, some deposits of fœces would escape observation altogether, and might remain for weeks subject to all the conditions found in exposure to the atmosphere.

137. If this be granted—and I do not see how it can well be gainsaid—the occurrence of isolated cases of dysentery in London is no longer to be wondered at. Nothing more is required than ordinary seasonal and other conditions for the exposed excrement to form the dysentery poison; and the persons coming within the sphere of its distribution will be affected. The marvel, indeed, is rather that there should be so few instances of empoisonment with the dysentery poison in such a large and varied community. I should have expected that the disease would have occurred endemically, far more frequently than it appears to occur in such neighbourhoods as those of the Docks. There, where Malays and Asiatics and men of all climes are congregated, must be ever present the sources of the poison; and it speaks well for the hygienic supervision of that portion of the great capital, that localised little epidemics of dysentery do not occur in consequence of the customs of the strange races herding there. For it may be observed that not only have Coolies, Lascars, and Orientals peculiar views with reference to their excreta, but many European nations are very heterodox, according to an Englishman's notions of cleanliness and decency. The result is that that quarter of London in which these foreign races mostly live, must be constantly liable to be infected with dysentery. That it is not more often subject to the affection is, in reality, one of the highest tributes to the efficiency of the sanatory arrangements of the district.

138. The great practical difficulty of providing against offences of this nature against society will always prove an impediment to the complete eradication of dysentery in a country. Epidemics may be provided against, as in England; but sporadic cases may always be engendered by individual carelessness, or filth, or through obedience to custom, or the laws of caste. A thoroughly

English town or city will never again be visited by epidemic dysentery, unless an extraordinary influx of people should take place and the privy accommodation prove insufficient—as happened here in Melbourne. Yet though serious and alarming visitations of the malady need not be apprehended where common decency is generally observed, still a few cases will inevitably occur even in well ordered and cleanly towns—more especially in the warmer latitudes. In England and the colder climes the effect of leaving excrementitious matter on the soil is more likely to engender typhoid than dysentery; but the nearer the line the greater the tendency of such exposure to dysentery.

139. This has been seen in Melbourne where the statistical returns continue to show a larger annual number of deaths from dysentery than occur in the whole of England. The greater mortality is the result, partly of the greater facility with which the dysentery poison is developed here during the presence of the seasonal conditions, and partly of the social surroundings of the place. Some of the beautiful public gardens and parks with which the capital of Victoria is surrounded have been converted into malarious grounds by the vagrant lazzaroni who live an out-of-door life. The evidence of the necessity for superficial scavenging will be patent to those who leave the pathways and go into the shady and less frequented portions of these reserves. Whilst this state of things lasts, there must always be the risk of inhaling the dysentery poison to those frequenting the gardens, and to those residing in the immediate neighbourhood, during the autumn and winter. The hitherto inexplicable cause of dysentery among the children of wealthy parents, or those of persons living in houses where all the hygienic conditions are perfect, is very easily to be understood, when it is known that their play-grounds may be in the vicinity of collections of excreta in all stages of putrefactive fermentation and covered with mildews that are sending their pestilent spores, filaments and mycelia into the air. In fact, if Melbourne citizens desire to keep these noble "lungs of the city," as they have been frequently called, pure and sweet and wholesome places of recreation, and to make them absolutely dysentery free, it will be necessary to scavenge the haunts indicated, or to dig in the excreta periodically, or to provide some efficient latrinal conveniences for the homeless. Nothing short of excluding the human excreta now left on the surface from prolonged contact with the air, can prevent each separate alvine deposition from becoming, under special seasonal conditions, an independent source of dysentery.

140. Perhaps the colonists most liable to be affected with dysentery are the stone-breakers. No class of men, in proportion to its numbers, suffers more from the affection. To those who are accustomed to travel much over the five or six hundred miles of metalled roads within a radius of twenty miles of Melbourne, and have seen these men at their work by the road-side, either singly or in little parties of two or three, or perhaps of eight or ten occa-

sionally, it may be a matter of surprise to learn that they are more prone to this disorder than other labourers. The fact would probably be set down to the exposure and to the damp and cold in their small calico tents on the waste ground by the fences. But damp and cold can never cause dysentery. The explanation is this. These men, who are much in the same position as those living without a settled habitation in the bush, have to leave their excretions on the soil. They resort to the nearest clump of trees, probably, and are perhaps restricted by the exigencies of their position to a limited area. The result is more or less surface accumulation of fœcal matter within a hundred yards, or less, of their tents. Supposing they remain in the neighbourhood for a few weeks—a much shorter time would suffice—and the seasonal influences favour the development of the dysentery poison, the chances are that, when the night dews carry the pestilential emanations into the atmosphere, currents of air will sooner or later convey them to the little encampments of the stone-breakers, who are thus stricken down with endemic dysentery on the most salubrious looking spots that it is possible to conceive. It is not long since that the police found a stone-breaker lying, in the last stage of exhaustion from the disease, by the side of the Eltham road at Heidelberg and took him to the Melbourne Hospital. Many other instances of virulent attacks of this flux under similar circumstances are of common occurrence.

141. The market gardeners round Brighton and elsewhere near Melbourne, are also liable to have more or less severe touches of dysentery in the autumn; and as they lead tolerably healthy lives generally and as their cottages are well ordered and the conditions surrounding them are favourable above the average, these periodical recurrences of endemic dysentery have been remarked upon by the medical men of the locality as being obscure. So regular is the return of the disease in the neighbourhood that when I enquired of a resident medical man this summer if there were any cases about, he said "Oh no—we shall not have any until the autumn:—then there will be some." Upon following up the enquiry as to the causation of these isolated instances of the malady in places apparently the least likely for their occurrence, I found it a very simple matter. The gardeners who supply the Melbourne market, mostly take back a load of manure—of stable dung chiefly, but now and then of night-soil. The human ordure thus conveyed to their gardens is not in a favourable condition to produce the dysentery poison, if shot out of the cart in a heap and left; and if spread upon the soil and dug in at once, it is incapable of producing it. But the probabilities are that occasionally the fœcal matters will be thrown over the surface and, either accidentally or designedly, allowed to remain for a time. Showery weather and a hot sun may then soon evolve the dysentery poison. And when all the conditions are not favourable to the development of the dysentery poison, the poison of enteric fever may be developed in its stead.

142. The volumes, quantities, or values, of the dysentery poison that may be disseminated in a given locality will depend, first upon the amount of fœcal matter exposed therein, and secondly upon the manner, or degree, of the exposure. Where the depositions on the soil have occurred in the natural way, or in the fulfilment of the natural bodily functions, the sum of the excreta and the period they have lain on the ground will determine how far the air shall be empoisoned, and the extent of the evil that shall follow. When the infection of the atmosphere is limited to the exhalations arising from the excretions of one or a few individuals, the germs of disease may light upon only one or two or three persons; or, in sparsely populated districts, may not reach one. When, however, there is a large accumulation of fœcal matter in close vicinage to great numbers of people, it furnishes the most potent, and, indeed, the only, means of bringing about those fearful epidemics which have appalled the world by their fatality. Night fogs and damp exhalations holding in suspension the poison carried up from the putrid excreta of thousands and hanging over a city, or an encampment, do the work of infection on a large scale. There is but little chance of escape for those who come within the sphere; and the nearer the source of the poison, the more certain the result and the greater the effect—though currents may waft a bank of poison-containing vapour a long distance;—as far, at all events, as from the rear of an army to the quarters of the general and his staff. The intensity of the effect of the poison on the human frame would seem to be in direct relation to the amount and kind received into the system. On this supposition only, making all due allowance for the variation in diatheses, is to be explained the greater or less virulence of the attacks upon individuals:—some of whom shall have comparatively mild symptoms of the malady, with the bowel discharges slightly tinged with blood; while others, in perfectly good health previously, shall die within twenty-four hours with a raging burning fever and with downright hæmorrhage accompanying the evacuations.

143. Although, therefore, it is quite possible that the ordinary excretions of one man, for one day, may generate sufficient poison to sow the seeds of a fatal endemic attack of dysentery, yet the larger epidemics are necessarily associated with more abundant material: and a careful study of all outbreaks will show that the malignancy of the epidemic has been in proportion to the quantities of fœcal matter and the more or less perfect submission of the mass to the conditions herein indicated. The utmost perfection of pollution is found when excrement is voided and left undisturbed on the surface of the soil in warm moist weather, or when the days are hot and the nights cool, and there is a heavy dew.

144. It now becomes a question how far disturbance of, or interference with, excreta, vitiates the production of the poison and lessens the amount given off. The principles upon which the due elimination of the active agent of dysentery seem to depend have already been indicated; and it remains to estimate to what extent

the quantities of the agent have been hitherto controlled by mankind. When nations resorted to expedients to obviate the unpleasant consequences of the accumulation of excreta on the surface, and to avoid the necessity of having to go long distances daily for the purpose of defecation, they introduced for the most part some rough contrivances which not only answered the purposes, after a fashion, for which they were intended, but at the same time interposed obstacles to the formation of the dysentery poison, to the same extent as theretofore. Every movement in the direction of cleanliness, comfort and decency lessened the amount of the poison in general circulation, in a direct ratio to the degree of the effectiveness of superficial scavenging. All artifices which reduced by so much the area of the surface of fœcal matter exposed to influences of the atmosphere, also reduced by so much the poison-forming capacity of the material. Hence the diminution in frequency and severity of epidemic dysentery in some countries and its total suppression in others.

145. Receptacles for night soil, therefore, diminish the values of dysentery poison evolved in a country, no matter how imperfectly they may be made as regards leakage into the ground. [No doubt the escape of fluid containing excrement may, under certain special conditions, be the means of adding to the pollution of dysenteric water; but this is a question apart, to be dealt with hereafter; and besides even admitting that fœcal matter does escape and does increase the degree of pollution of "bad water," it is still probable that the total quantity of effective poison produced would be less than by leaving the excreta on the ground:—so that the proposition is not materially affected]. Every description of cess-pit, midden, or privy, even mere holes in the earth without any provision against leakage into the surrounding strata, reduce the quantity of dysentery poison; and, when adopted by a whole community, lead to the total extinction of the flux. Tubs, pails, tinettes, boxes, commodes, and every other contrivance brought into use by means of which excrement is kept from the surface, lessen by so much the sum of the poison in a country. In short when a nation is so far advanced in outward public decency that all human excreta are removed from the face of the earth and disposed of artificially, so that they cannot be subjected to the conditions herein suggested, epidemic dysentery must perforce cease; for although sporadic cases may arise, especially in warm moist countries, owing to uncontrollable individual action, yet these exceptional endemic affections can never spread to any extent, or be converted into such epidemics as that at Marseilles, referred to by Trousseau. Such an outbreak, I undertake to say, could not occur from the introduction of troops suffering from dysentery into any town of England, no matter how badly drained or sewered. Of the large towns and cities, Liverpool has the highest death-rate—Manchester coming next. As sewage of fœcal matter in English towns governs the mortality, Liverpool is, inferentially, the most excrement-sodden town in England. And yet

it would be impossible to start an epidemic of dysentery there like that of Marseilles when the sick soldiers were brought from the Algerian hospitals. Bad as the hygienic arrangements of Liverpool must be, as compared with other large English towns, and especially as regards fœcal matter, still such is the state of the superficial area of the borough that an extensive and wide spread attack of dysentery could not have place. The pollution by excrement being mainly from below the surface level, dysentery is shut out. To show that this is not a mere hypothesis, or an unsupported assertion, I may point to the fact that large numbers of sufferers from dysentery in all its stages are annually imported into England from different countries. India, China, the West Indies, the ports of the Mediterranean, of the Black Sea and of all parts of the world are constantly pouring the disease on the sea-board of Great Britain. In the days of the late Russian war this regular, constant, steady, influx, received a material accession by the convalescents and invalids from the Crimea, numbers of whom died from dysentery after their return home. And yet we have never heard of the malady having laid hold of any of the populations among which it has been taken. The last appearance of dysentery in England in such proportions as to entitle it to the name of an epidemic was in 1831 at Bolton, where it succeeded the cholera. But nothing in the shape of an extension of the disease from the numerous centres which have been established at different periods in our large seaport towns, has been observed. The trade of Liverpool must cause a deal of exotic dysentery to gravitate there. Its foreign shipping is considerable; and therefore it must happen that numerous persons, both seamen and passengers, are landed with the affection, as in London. I have not met with any account of the dysentery cases imported into Liverpool, but in the *Lancet* for 29 July 1871, Mr. Clapham gives a tabular statement of fifteen cases in the Seamen's Hospital at Greenwich—all of foreign origin. It may be assumed then that Liverpool also receives its quota of dysentery. And yet the malady has not been known to spread, in spite of the fœcal pollution of its soil.

146. But a more decided case in point is that furnished by the return of the troops from Corunna after the renowned retreat. There the circumstances were as nearly as possible the same as in Marseilles—except that a larger number of English soldiers was landed at Plymouth than in the French town. In the Edinburgh Medical Journal for 1809 there is an account by Mr. R. Hooper, of the reception, treatment and mortality of those portions of the British force taken to Plymouth. It appears that 2432 men were received into various establishments in the town suffering from dysentery and *typhus* fever, and of this number 241 died:—though Mr. Hooper says the returns were only "up to the 27th of March and consequently do not include the amount of deaths beyond that day, though several must have died afterwards." He adds that 25 of those who died had wounds, but they had dysentery or typhus also. In the returns of deaths from the different places where the

men were attended to in Plymouth, Mr. Hooper does not distinguish between the mortality from the two diseases; nor does he indicate the numbers of those affected with dysentery and typhus respectively. He says the dysentery cases were the first to demand attention, which may have been from their urgency as regards symptoms, or from their importance numerically. And I should be disposed to take the latter view, as it is mentioned they were mostly chronic cases;—many of the men having contracted the disease in Spain some time before they embarked at Corunna. At all events a very large number of men were thrown into Plymouth suffering from this malady under conditions offering as close a parallel to the Marseilles case as can well be conceived. And yet Marseilles was attacked with a severe epidemic of dysentery, while Plymouth remained perfectly free from the scourge.

147. There is one important point which, obviously, has to be taken into consideration in comparing the relative results as regards the spread of dysentery at Plymouth and Marseilles; namely, the period of the year. This of course would be likely to affect the question materially. Trousseau does not give the season when the Algerian troops were received into Marseilles; but from the fact that the epidemic of dysentery which followed as a consequence of their reception was an exceptionally extensive one, it may be inferentially supposed that it was at a time when the conditions of propagation were most favourable; at the end of a moist summer, or the middle of a warm autumn, probably. It must be granted, therefore, that the arrival of the English troops in midwinter did not afford similarly favourable conditions for the propagation of an epidemic in Plymouth, even if the hygienic state of that town had been the same as that of Marseilles. Still it will be observed that the soldiers in Plymouth were dying of dysentery up to the 27th of March, and that "several must have died afterwards." This would indicate that a number of chronic cases were left, and that deaths among them continued far into the spring of the year, when the climatic conditions for the propagation of the disease were quite sufficiently favourable, supposing the other necessary conditions to have been present, to have involved the people of Plymouth. It is well known that the advent of winter in the Crimea did not lessen the quantity of dysentery in the army to any material extent. The disease is by no means precluded by cold wet weather. Therefore, it appears to me that the fact that dysentery did not take hold at Plymouth, or at any of the other towns and places along the coast on which the troops were thrown by the storm which dispersed the ships, shows that the surface cleanliness of England had then advanced to the stage at which an epidemic of the flux becomes an impossibility.

148. In order further to illustrate my position that the dysentery poison cannot be evolved from any amount or degree of pollution within the soil, I extract the following from the 12th Report of the Medical Officer of the Privy Council 1870. Dr. Buchanan and Mr. J. Netten Radcliffe report—

"In Birmingham only one lesson was learnt from the present inspection, viz., that a town may have the worst possible arrangements for dealing with its excrement, and yet have a credit for healthiness, and indeed, not have the very highest of all possible death-rates." * * * "At present it is common to find huge, wet, fœtid middens uncovered, undrained, unemptied, some of them as deep and as big as the foundations of an ordinary cottage. Few of them are covered, the Inspector of Nuisances thinking they are better left open."

In a more recent report as to the condition of Leeds, Mr. Radcliffe says—"The common privy, with midden-stead, is, in fact, still the ordinary provision for excrement disposal, &c., although long recognised as the filthiest and most unwholesome mode of excrement disposal, &c. Midden-steads of the construction here described, gaping wide to the air and rain, facilitate the soakage of their liquid contents into the soil around them. The disgusting state of the older midden-steads and of the soil into which they are sunk is indescribable. * * * It is actually a serious question whether these midden-steads are most dangerous when full or empty. When emptied, the filth-saturated floors and walls give off a greater amount of disgusting stench than when the receptacle is filled with mixed ashes, house refuse, and ordure."

149. If dysentery does not fasten on such towns as these, it seems fair presumptive evidence that alluvial malaria from the decomposition of organic matter in the soil is not the efficient cause of the disease. And if dysentery steadily declines and eventually dies out of a town in the face of the existence, and of the growing increase, of such pollution of the soil, it appears to me conclusive that the organic matters that may be in the soil have very little, if anything, to do with its causation. Neither Birmingham, nor Leeds, had large populations in the days when epidemic dysentery ravaged London, it is true; but the state of the soil in London must have been somewhat analogous to, if not worse, if possible, than that of the modern towns, when the system of sewerage and the general sanitary arrangements of the metropolis were commenced. It is clear that each succeeding epidemic in London must have found the city in a worse condition, as to soddenness of soil with excrement and other organic matter, than the preceding one. The last that appeared must have been under the worst conditions of all in this respect. And between it and the final work of sanitation, the soakage into the ground of London was going on at a vastly progressive rate. That dysentery did not supervene, and on a proportionately fearful scale, for many years before the hygienic arrangements of the city were commenced, is sufficient to show that alluvial decompositions are not materially concerned, either in its origin, or its propagation.

150. If it be supposed that although admixture with the soil, or removal from the surface, may destroy the power of fœcal matter

to form the dysentery poison in England, yet would not suffice in warmer countries, I adduce the cases of Melbourne and of the gold-fields of this colony. In both there was abundance of dysentery in its worst epidemic form, and for fifteen years there has been no outbreak, either in Melbourne, or on the old-established diggings. The conclusion, therefore, is forced upon me that all the devices employed by civilised man in dealing with his excreta, lessen the quantities of the dysentery poison—according to their effectiveness in the direction specified. Of course there are artificial modes of disposing of excrement by which dysentery is not precluded. Both civilised and uncivilised nations and individuals have peculiar ways by which, although they do not leave their fœces exposed on the surface, as voided in the ordinary way among all primitive races, they still render themselves liable to be attacked with the malady. But in all these instances, it will be found, I submit, that these people have acted in contravention of some one or other of the conditions laid down for the prevention of the formation and evolution of the poison. The Chinese, for instance, collect excreta carefully in receptacles, for manurial purposes; but they occasionally leave a thin layer on the soil. Some nations pass their stools into running water which carries the matter off, but no thought is taken as to its destination. It may be intercepted and prove a most efficient source of the poison. Some men build privies on the side of a hill so that the solids and fluids escape and are exposed to the air. Others have erected them on piles driven into the sand on a shore subject to the influence of tides. There are countries where it is the custom to empty receiving vessels—pails or tubs—over the edges of rocks, either into ravines, or into the sea; by either of which means collections of fœcal matter may take place on projections. The same principle obtained in the old feudal practice of ejecting the excreta from the castle over the battlements. In these and all similar artificial arrangements, it is obvious that dysentery is imminent; but I contend that reason shows and experience will prove, that when fœcal matter is disposed of genuinely in obedience to the suggestions thrown out, dysentery will be an impossibility. And that even in the hottest region, with the most excrement-sodden soil under the sun.

151. But I must be careful to steer clear of being misapprehended. In insisting so decidedly upon the certain eradication of dysentery by excluding fœcal matter from contact with the external air, it may, perhaps, be supposed that I am insensible to the other consequences that might result from the injudicious and ill-advised disposal of the excrement of large communities below the surface of the soil. It is not so, however. I have an acute perception of the evils to be apprehended from merely putting excrement out of sight, without at the same time adopting systematic and comprehensive measures to destroy its power of creating further mischief. It is not dysentery alone that has to be feared and provided against. It may, as I believe, be successfully com-

batted by the means suggested, no matter how rudely and carelessly they may be employed. But it is extremely doubtful whether the substitution of the privies and middens of these last hundred years in England has not been attended with as much loss of life, as occurred when our ancestors left their excreta on the surface. In getting rid of dysentery, it is not clear that an increase of other diseases just as fatal has not been a consequence of the mode by which it was got rid of. What has been gained in one direction by accident, has been lost in another through ignorance. The concession to public decency and surface cleanliness, which worked wonders in clearing the land of dysentery, was followed by private habits and underground pollution that possibly more than counterbalanced the physical good, by the increase of enteric fever, relapsing fever, and infantile diarrhœa, in which the change has resulted; and by the facilities it has afforded, as I conceive, to the reception of the cholera germs. And independently of these diseases, it is by no means clear that the diphtheritic poison is altogether disconnected from fœcal matter; or that epidemics of influenza and some other obscurely-caused affections may not be engendered, or favoured by excrement pollution of the soil of a country. More will be submitted on these points further on.

PROPOSITION IV.

That the poison thus formed passes into the atmosphere, and is capable of producing dysentery in those exposed, to a certain extent, to its influence.

152. This is a proposition of minor importance. For, if it be admitted that the poison is actually formed in the manner submitted, it will probably be conceded that the air is an efficient medium for its distribution. Whatever the substance emanating from excrement may be;—whether particles of the excrement itself in a certain state of change, or portions of fungoid vegetation growing on excrement—is immaterial to this question. In either of these cases, or in any other case, the air must, of necessity, be one great channel of communication between the source of the poison and the persons affected by it. There is no occasion, therefore, to demonstrate this part of the proposition. It has been submitted indeed, not for the purpose of showing what will be readily granted, but with the object of contrasting the simple mode by which the hypothetical dysentery germ is evolved, with the more complicated conditions supposed to be concerned in the production of the hypothetical choleraic poison. The dysentery germ is supposed here to be generated and perfected in its nidus, and to be given off into the atmosphere straightway as a complete poison, quite capable, without any further change before entering the body, of causing dysentery. But the cholera germ, according to Pettenkofer, requires the supposition of a more elaborate preparation before the ripe poison is formed. He assumes that the exotic germ, when

introduced into Europe, must first meet with a favourable substratum before it can be converted into the ripe poision. In fact he lays down that the germ must have a substratum in India and another in Europe, before it can induce cholera in the latter. I shall give his views more at length elsewhere, however, [196] and shall venture also to state mine upon the same point; when it will be seen that I offer a different explanation of the manner in which cholera is propagated through different countries.

153. The words "to a certain extent" in this proposition demand some little notice, perhaps, inasmuch as they are rather vague as they stand. It is impossible, however, to be more definite in this case. The extent of exposure to the poisonous effects of the dysentery germ necessary to induce an attack of the disease, is as variable, probably, as the units of the human race are variously fashioned. There is no predicating the result of a given amount of exposure in any individual case. And the reason why will be obvious enough, when it is reflected that any calculation of this sort has to be based on two unknown quantities—the precise quantity of the aërial poison in a given volume of air and the amount actually inhaled by the person. The apparent aberrations that occur in infected districts are very strange and at present unaccountable. Thus men living in the very centre of a dysentery epidemic for weeks shall escape and a visitor of a few days shall be mortally affected. No doubt these seeming anomalies are the outcome of fixed laws, though we cannot at present get at these laws. The only thing that can be done is to look at general results. We may form some tolerably fair conclusions upon the basis of large numbers of cases, but that is all. The "certain extent" of exposure to which allusion is made in the proposition is, therefore, hardly to be laid down. Perhaps the nearest approach to a definite understanding of the sense in which I have used the phrase may be arrived at by putting it in this way; namely,—when a person is submitted to such a degree of exposure to the influence of a germ-laden atmosphere as must ensure the introduction of germs into his system, they will cause the disease. But this is vague and unsatisfactory also; for it is impossible to say what degree of exposure must ensure the introduction of germs or what number, or what parts, of germs, will be effective in a given case, even when they are introduced into the system. In fact dysentery is on a precisely similar footing in this matter with cholera, typhoid and other fevers and zymotic diseases. It is no more possible to say what extent of exposure to the exciting cause is required to produce any one of these diseases, than it is to say how much exposure to the dysentery germ is sufficient to produce dysentery.

154. The conclusions I have formed with regard to the pollution of water by the dysentery germ, together with the further arguments to be brought forward in support of the theory just advanced upon the causation of dysentery, will be found at the end of the book. This plan breaks the connection of the subject. It looks

awkward, and impairs so much of the force of the reasoning as may be dependent on arrangement. But the inducements held out to me to postpone these portions of the work to the last moment before going to press, outweigh considerations as to mere method. A learned microscopist has at my suggestion entered upon certain investigations connected with the evolution of the dysentery germ; and as the results of his observations may modify my views and may serve to place the whole matter on a very different footing, I am induced to await those results as long as possible. At the same time the urgency of putting forward the views I hold at the earliest moment, so as not to lose the outgoing European mail in fact, impels me to get them into print at once. I will therefore leave the subject of dysentery now and pass on to speculations upon other cognate subjects which, though subsidiary to my original object, are nevertheless just as important to civilisation.

DISINFECTION.

155. It is not proposed to deal with particular disinfectants, or to touch upon the relative merits of the numerous agents that have been employed as such. The object in view is to deduce some general principles upon which disinfection, in its widest sense, should be based. And as I believe the views I hold upon the causation of many diseases to be peculiar, so my conclusions as to the philosophy of disinfection in connection with such diseases will be peculiar also. I have not thought it necessary, therefore, to consult all the works of those who have written specially on the matter. I have merely dipped into one or two of the more modern treatises; and, as I have not found anything to show that my vein has been struck hitherto, I shall give my reflections without reference to the views of others.

156. It will naturally be gathered from what has gone before, that I infer that the principal aim and object of disinfection, as regards zymotics, should be in the direction of excrement:—an inference which has been drawn by others, however. Almost all writers now recognise the paramount importance of this material as a cause of zymotic disease and, as a consequence, the necessity of depriving it of its dangerous properties. It is universally regarded by modern observers as a large, even if it be not admitted to be the largest, source, of one wide-spread and mortal fever; while it is more than suspected of being concerned in some way with the origin of other deadly plagues. The attention of skilled hygienists has therefore been forcibly drawn to excrement as a substance to be specially disinfected. This is evident enough from the Privy Council Report already quoted [148]; but inasmuch as it is material to the points about to be discussed, I will take another extract from a Report of Mr. Radcliffe, illustrative of his views upon fœcal matter. He says:—

"The sewerage of Leeds, in brief, while it has largely promoted the surface cleanliness of the town and suburbs, by the removal of house slops and rainfall, has only in an exceedingly limited degree tended to abate that chronic state of excrement-nuisance which has long been, and still is, a cardinal evil in that town and borough, and which it was designed to remove. Indeed, in respect to the drainage of midden-steads, it has been converted into an actual means of aggravating the evil. Any marked benefit to the health of the population which would have been obtained from the sewerage by the abatement of surface nuisance, has been altogether masked by the unwholesomeness of the graver and practically undiminished nuisance from privies." * * * "The prevalence of enteric fever is chiefly determined by the same local conditions which foster diarrhœa, namely the pollution of the soil in the vicinity of houses, of the air within houses, and of the water used for drinking purposes, with excrementitious matter. Excrement-sodden earth, excrement-polluted air, and excrement-tainted water are the principal factors of the production of these forms of disease. The extent of prevalence of typhus and of relapsing fever is mainly governed by the degree of overcrowding of families, also of houses in close courts and alleys, and by the amount of destitution." * * * "The causes which have given rise to this excessive mortality" [Leeds] "are—(1) In respect to diarrhœa and enteric fever: (*a*) An indefensible method of excrement disposal, namely, the common privy with midden-stead, constructed and perpetuated in its most offensive and dangerous form, although its mischievous effects upon the health of the population has been repeatedly indicated during the past forty years." &c., &c.

Mr. Radcliffe does not mince matters, and if the people of Leeds do not take the hint it will not be because it is so delicately veiled that they may not see it. There can be little doubt that the statements of the Medical Officer are literally true and his views correct in the main. It might perhaps embarrass him to have to show the precise mode by which excrement causes typhoid, or to point out good and sufficient reasons for excluding it from the causation of typhus and relapsing fevers; but Mr. Radcliffe has not at all events over-estimated the importance of human fœcal matter in its effect on the death-rate of a population. And when I say that I go far beyond him and believe excrement to be directly responsible for much more than he lays to its credit, it will be seen that, while devoting my attention to its relation with dysentery, I have not lost sight of its close and inseparable connection with other diseases.

157. Fœcal matter then has to be disinfected before all other things when the object is to stay an epidemic of enteric fever. This is supposed to be admitted. The question therefore arises:—how is excrement to be disinfected so as to deprive it of the power of causing enteric fever? Or in other words what is efficient disinfection of excrement, *qua* typhoid? As this question involves many knotty points, it may be as well to settle terms before going into it. The word disinfection is not sufficient to express all that

is required. Another word is wanted to convey a similar, but not the same meaning. I take disinfection to refer to the means used to destroy infection already existing in a substance; but the term cannot accurately be employed in speaking of the means used to prevent infection of a substance. A thing not infected cannot well be disinfected. Choleraic stools may be disinfected, but there is an incongruity, if not an absurdity, in speaking of the disinfection of healthy excrement—even though the same means may be used to destroy infection in the one case and to prevent infection in the other. There is no word that I know of which is capable of conveying the precise meaning required, though one has been coined expressly for fœcal matter. The term *defœcation* is now employed in describing the destruction, or breaking up, of fœces. But this is rather a technicality confined almost entirely to the phraseology connected with the excrement-disposal of towns. It answers its purpose, perhaps, though the principle of *idem sonans* was ignored by the inventor. But it is inapplicable to other organic material. It is impossible to talk of defœcating the offal of a slaughter-yard, for instance, or house refuse, or decomposing vegetable matter. There is still another word wanted to express the prevention of infection; a parallel word to disinfection. Until a better be found, the term *de-infection* will probably serve. It will be used in what follows, therefore, in the sense which has been sufficiently indicated, somewhat largely; for after all the question of the means to be adopted to prevent the origin, or to stop the spread, of infection, is of infinitely greater importance to mankind, than that as to the measures to be employed in the destruction of infection when it exists. The deinfection of civilised countries is, in fact, the largest question affecting human interests of the day. To avert entirely the evils surrounding fœcal matter is by far the most momentous social problem of England at the present moment. All the great towns are introducing as rapidly as possible systems of complete excrement removal; and the result is that the thousands of tons of excreta poured into the surrounding country have created nuisances in every direction, and led to injunctions from the Courts of Law and Equity without end. No one seems to know precisely what the dangers are, or the exact means to obviate them. The rudiments of the art of deinfection have hardly been learnt as yet.

158. How far are disinfectants operative? To what extent do they influence the quantitative amount of the poison of enteric fever, for instance, eliminated from fœcal matter. How far are these agents effective in arresting, or destroying, the poison-forming power of the material? As well as I can make out very little seems to have been determined up to the present, although a vast amount of experimental work has been done. The world of hygiene has evidently been thoroughly aroused to the importance of the thing, and has recognised the pressing necessity for finding some means of neutralizing the baneful effects of human excreta. But hitherto the positive knowledge gained does not appear pro-

portionate to the strenuous efforts made. Enormous sums have been expended in this direction. Gigantic works have been started and chemists everywhere have been busily employed, for years past, in testing the comparative merits of the various agents that have been suggested. Yet if stock be now taken of the result, it must be confessed that the sum of our experience is little more than this:—that many substances are capable of deodorising fœcal matter more or less successfully, with a greater or smaller expenditure of time and money, but not one, hitherto discovered, can be said authoritatively to have the property of disinfecting excrement in the true meaning of the word. That is to say there is no known agent of which it may be safely affirmed that when it has been applied to, or mixed with, infected fœcal matter, that matter is thereby rendered innocuous under all subsequent conditions. Of course I limit my observations to those agents which are generally included under the head of disinfectants and have been practically employed as such. It is not meant that excrement is absolutely indestructible by chemical means. The mineral acids would make it safe enough, unquestionably, and there are other modes of decomposing it and dispersing its elements, so that it shall be completely destroyed. But there are weighty considerations against resorting to these means. Speaking simply of what are recognised as disinfectants, then, I repeat there is not one of them in use of which it may be confidently said it is a disinfectant. I observe that while Professor Maclean enjoins that dysenteric stools should be received in glazed vessels into which some disinfecting solution has been poured, he nevertheless insists on the burial of the vessel at some depth. I find also that in India gaol fever, according to Dr. Rolleston, [Lancet 1869] spreads, notwithstanding the excrement of prisoners is received into earth-closets and is thoroughly deodorised. The Oxford Minute [Lancet 1871] is very cautious touching the complete disinfection of organic compounds by the various materials alluded to therein. In fact after searching for some definite information on the point, I find nothing but doubt and distrust as to the effectiveness of these so-called disinfectants. Everybody seems to eye them askance, as though they had half a suspicion they were futile, and another half that they might possibly have something in them, and it would be just as well, therefore, to be on the safe side. This is somewhere near the present position of disinfectants, as I take it, in the minds of the scientific. At least that is the conclusion I form from the latest current literature available. It is equivalent no doubt to saying nobody appears to know much about disinfectants, and I may as well say plainly I suspect that to be the case.

159. It seems to me that the question of disinfection has been dealt with on the whole somewhat empirically. It has not been approached in a philosophical spirit and the subject has drifted into confusion. No one would appear to know precisely what he is aiming at. The object is plain enough, but its nature does not seem to be evident. The first thing to have been done, it strikes

me, was to have got a tolerably clear conception of the principles upon which disinfection should be based. And here the scientific world have failed, as I think, and have expended a deal of fruitless energy in consequence; or rather I should say they have employed their time and spent much treasure in things not immediately serviceable, or available for the special purpose for which they were designed. It cannot be said that any collection of facts in art or science, however costly, is fruitless. Perhaps if I were to ask those who have experimented with disinfectants, whether they had proposed to themselves the precise thing they undertook to do before commencing work, it might be deemed impertinent, or bordering on the offensive. If, however, I got a reply, it would probably be something to the following effect: namely, that the object, or objects, in view were to decompose, by oxidation or otherwise, all organic material in air or water that might be given off by the substance (fœcal matter only is now in question) and might prove deleterious to health; to destroy the substance itself at once, or else to break up its component parts, so that it should not give off any more organic material of a like kind; to intercept, arrest, or prevent the formation of, obnoxious gaseous products from putrefactive or other forms of fermentation, or from any species of decomposition, or change; and to extinguish all malodorous emanations.

160. This might be the substance of the reply and it is sound enough as far as it goes. But if the matter be pushed a step or two further, I think most, if not all, philosophers would find themselves at a loss for answers. For instance—what are the deleterious organic materials given off by fœcal matter? and how, or in consequence of what, are they given off? If I mistake not there is not a human being who could, at this moment, give replies to these questions that should even decently approximate to scientific accuracy. Quantitative analyses have been made of given volumes of air from fever-stricken wards and pestilential places, and organic chemistry has been enabled to determine the relative amounts of C, O, H, and N they contain. The result has been that an excess of organic material over the normal standard has been shown. Microscopists, physiologists, and others have also been at work to make out the nature of those minute organisms which poison the air. But whether from the inherent difficulty of the investigation, or from the conviction that the end was not worth the pains, the problem of the fever-germs in the atmosphere of excrement-sodden localities has not yet been solved. The chemist has stopped short at his residuum and the microscopist has not got far beyond the cellular bodies he has succeeded in discovering. There is nothing to show whether the organic particles floating in the air of such towns as Leeds are wholly of animal, or wholly of vegetable, origin; or are partly derived from one source and partly from the other; or, if the latter, which of the two—the animal or vegetable organism—preponderates, and in what proportion. Neither has it been demonstrated how, or

the process by which, these minute cellular bodies, of whatsoever kind they are, have been detatched from the substance of which they formed part.

161. Yet all these points must be resolved before disinfection can be put on a thoroughly satisfactory footing. They are of the very essence of the thing; and I venture to say that until they are cleared up, either by practical proof, or by sound induction, or both combined, all tentative efforts to find efficient disinfectants for general use must be uncertain in their aim and are not unlikely to be useless in their result. Nay more, they may be dangerous also, by inducing a feeling of security for which there is no warrant. It seems very like putting the cart before the horse to proceed to disinfect, before ascertaining the nature of the infection. I do not say that all that has been done has not been in the right direction. Every step that has been taken may have been straight for the goal, for all I know, or anybody else knows, to the contrary. But what I submit is that while the present nescience on the matters glanced at exists, the whole subject is in the realm of empiricism; and there is no saying whether or not all the legislation which has been effected in disinfection and deinfection and all the sums which have been expended have fallen short of, or gone beyond, requirements. In fine the matter is in that unsatisfactory transition state in which, I apprehend, no man having a regard for his reputation would be found to affirm that any agent, or agents, capable of being practically used, will disinfect the air surrounding persons suffering from infectious diseases, together with their evacuations, so that there shall be no danger from either source to those in attendance, or to those who may subsequently come within the sphere of the influence of the discharges from the body. Nor on the other hand, could such a man state confidently, or with any show of reason, that there may not be agents that might do all this. And since no one knows the nature or source of the specific poisons, who can say that a disinfectant in one case is an universal disinfectant in all cases? Who so bold as to declare that the disinfectant for diphtheria—if there is one—must be the disinfectant for small-pox or cholera? Who can premise that special modes of disinfection may not hereafter be found necessary in special diseases?

162. In fine there has not been, up to the present, any rational disinfection, or deinfection. For these imply a foreknowledge of the causation of the diseases against which hygienic measures have to be taken. As there is no instance in which this knowledge has been gained in Europe there can have been no scientific sanitation there, no matter how elaborate, costly, and effective in the reduction of a death-rate, the work hitherto done may have been. London is not yet scavenged, drained and sewered on true, sound, thorough, principles; and will not be until the present Art of Hygiene has been advanced to the position of a Science. This may be an unsatisfactory conclusion, yet there's no escape from it, and it may as well be looked straight in the face.

163. As the first essential step towards disinfection and deinfection is through causation, I will contribute my mite to the elucidation of this matter in connection with a small, but important, group of diseases. It will be understood that my object is to illustrate the spirit, rather than the mode in which, as it seems to me, the subject should be approached. I am sensible of the defective arrangement of the materials employed, but I also know that this is a trivial thing, which will be readily condoned if there be any back-bone in what I have to say. If not, no amount of order or method will redeem, or atone. The matter and not the manner, therefore, is what I must fall back upon.

ON

DISINFECTION AND DEINFECTION

IN RELATION TO

TYPHOID OR ENTERIC FEVER.

164. As this fever is, perhaps, the most largely fatal of all disorders on the list of epidemical dieases, I will commence with the consideration of typhoid. Most writers place dysentery first as regards the mortality caused in the world by one disease. But, for reasons which will be more apparent hereafter, I should be disposed to conclude that the typhoid poison has been the cause of more deaths than the dysentery poison. But the question now is —whence comes the poison of enteric fever? And again what is it? It seems agreed on all hands that fœcal matter is somehow mixed up with the causation of this disease. It has not yet been recognised as the sole cause, it is true, but I apprehend the time is not far distant when it will be seen that without human excrement there can be no enteric fever. However, in the meanwhile, I will only assume it to be admitted that the views of Mr. Radcliffe embody the general belief as to the connection between excrement and typhoid—" Excrement-sodden earth, excrement-polluted " air, and excrement-tainted water are the principal factors of the " production of these two forms of disease" [typhoid and diarrhœa] Here then is a starting-point. All efforts in the way of disinfection and deinfection for enteric fever must be directed to fœcal matter. It has to be met and counteracted in earth, air and water. Leaving out of consideration the disinfection and deinfecof the sick room, I go at once to the problems of the disinfection and deinfection of excrement in those places in which the typhoid poison originates. The solution of these problems must of necessity be preceded by the solution of that of causation. I will be as brief as possible, and, indeed, I shall have to take some things for granted which may be questionable, but which I have not time to stop to discuss.

166. *Excrement-polluted air*, in its relation to enteric fever, I propose to take first [*Excrement-sodden earth* is rather a medium by which air and water are poisoned than a direct means of creating typhoid.] The pollution of the air resolves itself into (*a*) *gaseous products*, (*b*) *animal organic matter*, and (*c*) *vegetable organic matter*. (*a*) Chemical combinations of any kind are incapable of causing enteric fever. Gases resulting from the decomposition of

organic matter may cause nausea, vomiting, diarrhœa, general cachexia, or sudden death; but they cannot, so far as is known, create a contagious disease. (*b*) The animal substances in recent fœces consist principally of portions of muscular fibre, cartilage and fibro-cartilage cells, fragments of elastic tissue, and the cells of fat, all derived from the food, and varying in quantity, therefore, according to the diet and more or less perfect digestion. Besides these educts there are some animal matters added to the fœces *in transitu*—chiefly mucus and epithelium, and perhaps some fat compounds from the bile. In addition to these normal constituents, fœcal matter may contain the ova of animal parasites, or the parasites themselves, or both, either living or dead. When excrement is exposed to air it may receive an accession of animal matter from the insect world; and in fluids it may get it from infusories. [I omit the specific typhoid poison itself.] The question is whether from any one or more of the sources indicated, air can be polluted with floating organic particles having the infective principle of enteric fever. The experience of the dissecting-room appears to dispose of atmospheric pollution from animal matters in all stages of decomposition. It seems to preclude the possibility of enteric fever from any form of decay in the tissues of the body. Besides there are many trades connected with putrifying animal matters of all kinds, and it has not been shown that the workmen employed have been more than usually subject to typhoid from exposure to the effluvia inseparable from their occupation. If in addition to these we consider the special business of the nightman, and bear in mind that his class is exceptionally healthy, it seems conclusive against the presumption that particles of animal organic matter, suspended in the air in any shape or state, are the efficient causes of typhoid. With regard to intestinal and internal parasites and their ova. If dead, the foregoing applies: if living, I do not see how they can affect the atmosphere. No entozoon that I know of can infect air by ova; and if it could, and the ova got into the organism of man, they might develope, but they would not induce typhoid. The only other source of air-pollution from animal organic material is that of the parasites settling on, or attracted to, excrement, after voidance. And here, where so little is really known, some reservation is necessary, and some caution must be observed. That there are aërial living animal organisms has been shown. Animated sacs consisting of a few cells, or possibly of but a single cell, have been found to pervade the atmosphere in many localities. The laws, however, which regulate their development and distribution have not yet been established. Microscopists and others have not demonstrated that special animalcular bodies are attracted to, or developed in, particular substances, though the probabilities are that it is so. The subject is pertinent to this enquiry in so far only as it relates to the air in the immediate neighbourhood of excrement; and with regard to it, I am not aware of any special investigations with the direct object of determining whether specific animalcules are to be found in the vicinity of fœcal matter, and in such vicinity only. It is not a wild or fanciful supposition that peculiar animal organisms collect about, or are given forth by, human excrement, and that they derive their pabulum therefrom. But the point cannot now be determined—at least I know of no authority by which it can be settled. However, the present question is whether animalcular agency, of any kind, conveyed into the body by way of the air, can, under any circumstances, produce enteric fever. The common animalcules clearly cannot cause the disease; but do those having their nidus in excrement, or fostered by the exhalations from that substance in some state of decomposition, create typhoid? It appears to me they do not. I think if they did, there would be some trace of their presence in some of the organs, or in the blood. I cannot conceive it possible that they should have escaped the notice of microscopists and others entirely. If these animalcules invaded the body, maintained their vitality, and reproduced themselves by gemmation, or cellular segmentatation, or any other form of cell division and growth, there would surely have been some evidence of it. The only investigator, I believe, who has connected animalcular bodies with the origin of a specific disease analogous to typhoid, is Dr. Dyes. He appears to consider them to be concerned in the causation of dysentery. The mere fragment of his views before me [16] precludes all knowledge of the steps taken by Dr. Dyes to arrive at his conclusions; but I assume he satisfied himself that the parasites in the mildew of plums actually pass through the stomach and are to be found in the intestines in an active living state. If the observation be confirmed by microscopists, it will establish that such animalcules do exist in the body. And now the question arises whether aërial animalcules also become parasitical after gaining an entrance into the system by way of the air-passages. I apprehend not; although the question is an open one, and, theoretically, the assumption that animalcules may effect a lodgment in this manner and may cause some forms of disease, is not a violent one. We know that many living parasites are found in the human body and one more especially has frequently been found in positions—in the brain for instance—into which it could hardly have got, but by way of the circulation. These known parasites are supposed to have found their way into the organism principally if not wholly by the stomach; but there is nothing to show that atmospheric animalcules cannot enter through the mucous membrane of the air-passages. If a coccus can get into the vessels and go with the blood to the brain and produce hydatid cysts, I do not see, theoretically, why animalcules from the air might not get into and exist in the blood as well. Still although it is just within the bounds of possibility that enteric fever may be caused in this manner, I do not take that view of its causation, for the reason that there is no evidence to support it, and because I believe there is another sufficient, and more rational, explanation of the means by which the affection is brought about. For my part, I discard from the causation of typhoid, both *gaseous products* (*a*) and *animal matter* (*b*) introduced into the body from the air in any shape or form.

167. (*c*) It is in the vegetable world that I believe we may eventually find the poison of enteric fever. Air containing typhoid infective matter may have received it—1. *from excrement unmixed with other material*; 2. *from excrement mixed with solid material*; 3. *from the surface of fluids, or semi-fluids, containing excrement.*

168. I. *Excrement unmixed with other material.*—The remarks made elsewhere indicate the nature of my views upon this point, and shadow forth some of the grounds upon which I base them. It will be sufficient to say here that I conclude that air is polluted in the regions where excrement abounds, by some form of mildew. Almost the same arguments by which I arrived at the source of the dysentery poison, might, I conceive, be employed as to the source of the typhoid poison. If I am right in the one case, it follows almost to a certainty, that I am right in the other. I believe that the mildews of excrement cause both dysentery and enteric fever. Whether the mildew of dysentery is distinct and separate from that of typhoid, or whether it is the same cryptogam in another stage of development, I will not pretend to say; but that the two diseases are caused in a like manner by a vegetable parasite growing upon human excrement, I have no shadow of a doubt. I cannot stay now to follow out the subject and to suggest the conditions under which excrement may, or may not, evolve the typhoid germ. I must leave it to those in a better position to do so; merely observing that I suspect there will be found but little divergence between the factors of the two affections. There are good reasons for assuming that the typhoid poison is developed earlier than that of dysentery; although the period of incubation is so much longer in the former disease, that it may happen, especially in warm climates, that dysentery breaks out first in an epidemic. It would also seem that the typhoid mildew may be developed under conditions not favourable to the growth of the dysentery mildew. There would appear to be an arrest of development from some disturbing cause, so that the vegetation stops short. Possibly less moisture, less exposure, and less warmth may suffice to evolve the mildew of typhoid from excrement. But I cannot prosecute the enquiry further at present. I will merely say that, inferentially, I conclude that air may be polluted with the germs of enteric fever from vegetation occurring on *excrement unmixed with other material.*

169. II. *Excrement mixed with solid material.*—The solid matters with which excrement is commonly found mixed in towns are ashes, house-rubbish and offal, refuse of all kinds, including stable manure and the dung of animals, and, lastly, earth. In every case it is abundantly evident that pollution of the air with enteric poison may take place. And herein lies the essential difference between the causation of typhoid and that of dysentery—a difference which is all important as regards the means to be adopted for the prevention of the two diseases. It is this distinguishing

characteristic which has led, as I believe, to the extinction of the one, and to the increase of the other, in England. Assuming a mildew on excrement to be the cause of typhoid, I conclude the admixture of its substratum with other solids does not interfere with its development to any great extent. This would seem to argue that fœcal matter is capable of supporting the typhoid mildew without undergoing great putrefactive or other change; whereas the dysentery mildew possibly requires a certain degree, or a particular form, of decomposition. However this may be, the fact [?] remains that the mildew of typhoid is not destroyed, or precluded, with the same facility as that of dysentery. The disinfection and the deinfection of excrement, as regards typhoid, are vastly more difficult and complicated problems than in the case of dysentery, and moreover involve more elaborate and costly arrangements. It is to be feared indeed that the fever will not be so readily stamped out, as the flux has been, in England.

Leaving excrement-sodden ground for the present, it appears to me probable that the mildew forming on excrement mixed with solids, occurs only on those portions of fœcal matter to which the air has access, and that complete occlusion from the atmosphere would prevent that form of vegetation by which the air is polluted. If the solids be moist, however, and if the sporules, or filaments or perhaps portions of the mycelium of the cryptogam, have found their way into the mass, it may happen that drainage from it may taint water. [173.] In places where the sporules are largely disseminated, it may readily be conceived that the mildew may spread rapidly wherever excrement is exposed; and it may also be understood how easily portions of mildew may be included in any heap of solid material, where it may vegetate in another shape, although excluded from the external air.

170. There is one strongly marked distinction between the poisons of typhoid and dysentery, which inclines me to the view that their germs come from essentially distinct, or specific, parasites, and not from the same cryptogam in a different phase, or stage, of development. I refer to the different sets of symptoms produced in the body by the two poisons. Elsewhere I have touched on the reasons why it appears probable that the dysentery germs cause their special lesions by mechanical means, or through some elective affinity for the glands in the colon, rather than by any specific poisonous material derived by the mildew from fœcal matter. [113, &c.] In the case of the fever germs, however, it seems more than likely that some acrid narcotic principle has been developed. The whole train of manifestations in those affected is that of blood-poisoning, and presents a great contrast to the clear mind and alert manner of those suffering from uncomplicated dysentery. If the typhoid symptoms admit of the explanation offered, it may further be understood that the vegetable poison eliminated by the *Mucor*, or whatever other kind of mildew it may be, from human excrement, may vary in its degree of virulence according to the nature or condition of the material, to seasonal

influences, and to difference of latitude. [253] Nothing is more clear than that the products of vegetation are modified by soil, moisture, heat and actinism. Therefore the narcotico-acrid principle of the typhoid germ may be modified. If this be so, it suggests a simple explanation of the ever-varying features of enteric fever in the same country at different seasons and at distant epochs, as well as in widely-severed regions. It throws a light on the anomalous symptoms and singular complications met with in this disease—symptoms and complications so unusual that many observers, although recognising its generic type, have concluded there must have been some specific difference between the typhoid under their observation and the typhoid found elsewhere and at other times. This diversity has been more especially noticed since medical men have got a more extended knowledge of the diseases of all quarters of the globe. The experience gained in this way shows that while some of the features of enteric fever may be present during the progress of an affection of a febrile nature, yet there may be such a marked absence of others usually considered diagnostic, that the physician has had great doubt about the true nature of the disease before him. But there is no occasion to go out of Europe to find almost startling peculiarities in enteric fever. Not to mention the uncertainty as regards the appearance of the characteristic roseolous eruption and the general symptoms and internal lesions, there is that curious phase of the affection, known as *typhus ambulatorius*, in which the patient may have an unclouded brain and may be able to attend to his ordinary avocations—suffering only from mild diarrhœa with a general feeling of uneasiness, or *malaise*, perhaps. Suddenly while at work, or going about, the bowel is perforated by an ulcer and death occurs rapidly:—the autopsy showing all the usual intestinal changes, as in enteric fever in an advanced stage. Such cases as these, where the local manifestations of the disease occur without the usual signs of blood-poisoning, are closely allied to dysentery. The subordination of the febrile disturbance points to the introduction of typhoid germs which, while perfectly capable of producing their topical lesions, have yet been deprived by some means of their toxical qualities. And between such rare instances of extreme aberration from the common type of typhoid and the well marked cases of enteric fever, the shades and grades of difference are innumerable. Epidemics and sporadic cases are varied infinitely in the same locality at different periods, and even at the same period. Sometimes the head symptoms predominate and sometimes chest complications manifest themselves. Now the characteristic spots appear in almost every case, and now they are always absent, except perhaps in some isolated instance out of hundreds—as it were to mark the eccentric nature of the disease. And yet modern enquirers in Europe have generally connected every outbreak of the fever, in all its many forms, with human excrement. Nearly every author has succeeded in tracing the causation of enteric fever up to fœcal matter—some of them undesignedly. To be sure some observers have discovered an efficient cause for certain local endemic attacks resembling typhoid, in rancid sausages, smoked puddings and putrid meat; but their views have been disposed of by Niemeyer and others. When indeed it is found that typhoid may and does occur independently of putrid food, it settles the question at once, and that source of the poison may be excluded forthwith. In fact all the more recent views converge to night-soil and sewage; and excrement is now almost invariably associated somehow with this disease. No one has demonstrated the precise mode by which the infection is conveyed, but most are agreed that fœcal matter is a principal factor. One of the important points that have militated against the reception of the idea that the origin of the poison is to be found solely in excrement, has probably been the Protean character of the symptoms of the affection produced by the poison. It has been difficult to reconcile the diversity of manifestation with a similarity of causation. Yet, if the view I have taken be sound,—namely that the specific poison of typhoid comes from a mildew on excrement; that that mildew possesses poisonous qualities; and that those poisonous qualities may be modified in the way suggested:—if this view be substantiated, all obscurity vanishes. On this supposition all these and other apparent discrepancies and incongruities may be brought into accord and made to harmonise. Even the singular *typhus ambulatorius* will square with typical enteric fever, and every intermediate grade between them is clearly explicable by the theory of an agent derived in all cases from the same source, but differing in potency, or activity, according to the conditions by which the source may be affected.

171. There is a point in connection with this part of the subject to which I must briefly allude. While I believe that the particular, or special, mildew which causes enteric fever originates on human excrement, as I have stated, I think it not improbable that it may extend to and overspread the dung of animals in contiguity, or propinquity. It may even affect all putrid or decomposing organic matters in its immediate vicinity; some of the low vegetable organisms undoubtedly display a facility in accommodating themselves to the different substrata on to which chance may take them, and it may be that the mildew in question is one of these. It is even possible that it commences on some other substratum than excrement, and that not until its mycelium is established on this material, does it acquire its most virulent properties. But, for many reasons, I take the view that it starts in existence on fœcal matter. Assuming it to be a *Mucor*—*pro hac vice*—and that a modified *Mucor* springs up from it on other substrata near at hand, the variety in the vegetation may possibly cause a variation in the symptoms produced in the organism of those infected. There is no reason why the sporules, [if such vegetation goes on to fructification], or at all events the cellular parts, of such mildew, may not enter the system and, according to the nature of the juices it may have eliminated from the substratum, modify the disease. If

there be any foundation for this notion, it will open up a wide field for investigation, and may serve to reconcile a whole host of anomalies in typhoid. Dunghills and decaying animal and vegetable matters may thus become accessories to human excrement, and may help to cause those milder forms of fever, sometimes called "febricula" and "mucous" and "gastric," which occur during the prevalence of an epidemic, but on the outskirts of the infected district, and would hardly be associated with typhoid, but for the neighbourhood, or except for some isolated case or two in thinly-populated places of a more serious and more decidedly typhoid type. The proportion of excrement to other substrata may possibly govern, to a certain extent, the nature of the resulting affection.

172. *Excrement-sodden soil*, according to Mr. Radcliffe, is one of the principal factors of enteric fever. The material question that arises, is, in what way it causes the fever. It is incapable when completely sodden of empoisoning air with the dysentery germ, as I contend, though it may lead to the pollution of water with that poison. But earth containing fœcal matter is an efficient cause of enteric fever, by air and water both. How is this? I need not refer here to the numerous theories which have been submitted to the world by others, but will at once proceed to adapt my mildew views to this branch of the subject of causation. As air-pollution and the tainting of water are inseparably connected in reality, and as the two processes may be going on at the same period within a short distance in the same vertical portion of ground;—the exposed strata, in fact, giving off atmospheric germs, while the subjacent and submerged layers are creating water germs—I will consider the two modes of propagation together. This is anticipatory of the dysentery germ in water; and as I may have more exact knowledge of dysentery-tainted water before this issues; and as the same principles of water-pollution probably apply to the whole of these diseases; I may have occasion to modify my views somewhat at the end of the work. Right or wrong, however, I cannot stop now, but will give my hypothesis as I have arrived at it—without data.

173. In the first place I have an unknown mildew to deal with. Although I have made every effort to ascertain the mildews developed by human excrement, I have been unable to determine them. The works of Fries, Tode and Cohn are not in this colony, I believe. They may contain references to these special parasites; but of the other mucologists I have had the opportunity of consulting, only one—the Rev. J. M. Berkeley—alludes to fœcal parasites, and that in a cursory manner. Mr. Berkeley merely indicates of two agarics that they occur on excrement. My argument therefore is admittedly purely hypothetical, and as such I advance it. I assume a mildew on excrement, capable of causing enteric fever, to begin with. This being granted, the pollution of water lies in a nutshell. Excrement in earth subject to the rise and fall of ground-water, becomes mildewed on its exposed sur-

face. In the case of an excavation in the ground, the mildew creeps along its sides and establishes itself, wherever it finds its substratum, down to the water-level. If the water falls, and thus lays bare a larger surface, the mildew follows it down and takes possession of the portions of excrement thus exposed. So long as the water recedes, the mildew is *a terrestrial mildew*; but when the water rises and the mildew is submerged, it takes the form of *an aquatic plant*, and adapts itself to its altered conditions with that facility which is observed in many low vegetable organisms under similar circumstances. It retains its vitality under water; and though a material change takes place in its mode of growth and propagation, it yet continues to reproduce itself by some kind of cell division until the material on which it depends is exhausted. Whilst its substratum is to be found in the soil, or the water permeating the soil, this aquatic mildew will probably flourish, attached either to the excrement still adhering to the earth, or to that suspended in the water. The oscillation of ground-water, therefore, governs the extent and prevalence of an epidemic of enteric fever in a most material way. In droughts, the subsidence of the water leaves a free superficies to be occupied by the terrestrial mildew of typhoid, which thus poisons the air largely. In heavy rain-falls, or when rivers rise from the the melting of snows and the ground-water surges up in consequence, the existing mildew becomes covered and is rendered inoperative as regards pollution of the air. It is only efficient in the empoisonment of drinking water at that period; and provided it does not find its way into reservoirs used for drinking, it is harmless. When the water sinks again, the former position of affairs is resumed, and the mildew on the excrement-sodden free surface takes on its normal character once more—producing a greater or less degree of air pollution, according to the rate of the fall of the ground-water and the amount of surface consequently left exposed.

174. Professor Pettenkofer and Dr. Buchanan had some passages in connection with this subject about two years ago, and came to opposite and apparently irreconcileable conclusions touching the effect of variations in the ground-water on the increase, or diminution, of enteric fever. I take my information as to Pettenkofer's views from the New Sydenham Society's Retrospect for 1869-70, p. 504. I extract what specially bears on the matter. "(2) In Munich, of all the momenta accessible for the investigation, the oscillations in the ground-water best show an unmistakable connection with the intensity and extension of typhoid. "(3) So long as the ground-water steadily rises, the number of deaths from typhoid steadily diminishes; but when the former is falling, the latter increases. (5) The fluctuations in the number of cases of typhoid compared with the fluctuations in the ground-water, after elimination of the yearly periods, enable us to recognise a coincidence showing a probability of 36,000 to 1, that there is a constant connection between the two phenomena. "(6) Further, all investigations show that, in Munich, in a month

"in which there happens to be an excessive rain-fall, there is a "decidedly greater probability of a decrease in the typhoid cases "below the average than an increase, and *vice versâ* in a dry "month. (9) Should it be imagined that the two events do not "depend upon each other, but upon a third and unknown factor, "we must suppose in the cases in question that the height of the "ground-water, the amount of rain-fall, and the frequency of "enteric fever, are governed by this hypothetical unknown factor, "and are in accord with it; &c. (12) Poverty, improper or unwholesome food, cold, uncleanliness in and about the house, "defective water-closets and sewers, damp, badly ventilated, over-crowded dwellings, marshes, &c., cannot explain the seasonal "variations of enteric fever in Munich. (13) Three great epidemics "of enteric fever have broken out in Munich during the last fourteen years, during which period the state of the ground-water "has been observed. The severest epidemic, in 1857-8, coincides "with the lowest level of ground-water; the next in severity, in "1865-6, with the next deepest; and the third in severity, in "1863-4, with the third level in depth of ground-water. (14) The "same law comes out in the reverse case. Munich had the smallest "typhoid mortality, since 1856, in the year 1867, at the time of "the highest level of ground-water; &c. (15) The influence of "different potable waters upon the prevalence of enteric fever in "Munich has failed to be in any way established."

175. Dr. Buchanan challenges these views of Pettenkofer, and attributes the increase of enteric fever which occurs during the subsidence of ground-water, to the larger pollution of drinking-water caused by the subsidence. He does not recognise the necessity for the unknown factor (9) suggested by Pettenkofer. Without attempting to reconcile the statements and conclusions of these two authorities, it appears to me quite possible that they could be reconciled by the mildew theory of enteric fever. What Professor Pettenkofer says of the phenomena observed at Munich is readily explained. The "hypothetical unknown factor," I take to be an unknown mildew; and, on the assumption of its veritable existence, all the observations of the Professor are clearly to be understood, except perhaps the last one (15). If the drinking waters of Munich do not affect the amount of typhoid in that city (which is doubtful I should say), it can only be because the excrement contaminated waters of the place do not communicate with the drinking-waters. What Dr. Buchanan remarks, as regards the subsidence of ground-water polluting the wells, may be quite correct; and yet the facts he mentions are not conclusive as against the correctness of Pettenkofer's views. In fact, should the mildew theory, herein advanced, be found on examination and by actual proof, to be sound—and it admits of being practically tested without difficulty by those who are conversant with such investigations—there will be at once a means of showing, what I suspect to be the case, that both sets of observed facts may be made to square, however much opposed they may now appear.

176. Although only one mode of water-contamination from excrement-sodden earth has been given, it need scarcely be remarked that there are innumerable ways by which water may be tainted with the typhoid germs; all of them depending, however, on the conversion of the terrestrial mildew into an aquatic form of vegetation. It may further be supposed that, as in the case of the probable extension of fœcal mildew to other substrata in its vicinity [171], the aquatic plant may seize on other organic material, as well as excrement, in water: and may maintain itself [though with some alteration perhaps of its poisonous qualities] so long as any substratum is within reach. Thus drainage from a pig-sty, or a farm-yard, or a slaughter-house, may assist in supporting the typhoid germ in a modified state in water. The vegetable growth will probably select excrement as its natural substratum; but in the event of the excrement being broken up or exhausted, it is not improbable that it will then make a struggle for existence by fixing on other material. This is an analogous mode of inducing strange varieties of typhoid to that before suggested, supposing the water containing the altered fever germs to be drunk.

177. III. It now remains to dispose of the question whether air receives typhoid infective matter *from the surface of fluids or semi-fluids containing excrement.* This opens up a large subject, including, among other things, considerations as to the infective qualities of the atmosphere near privies, cess-pits, middens, water-closets, earth-closets, and all kinds of excrement receptacles. Some of the points have been already touched upon in dealing with the evolution of the dysentery poison. Yet it may be advisable to enlarge. In the first place I think it may be safely affirmed, that effluvia coming from the contents of privies are altogether incapable of producing typhoid. By way of illustrating the point, I may refer to the exemption of night-men. This fact is well known, in consequence of the statistics procured by Dr. Gull, as to the night-men of London, who appear to be the healthiest class in the capital. Indeed it is perfectly evident that persons much exposed to the effluvia from fœcal collections in these receptacles, no matter how long they may have been accumulated, do not suffer. They may and do follow their occupation without fear of fever infection from this source. These collections of excreta then do not appear to give off emanations which are efficient causes of fever. Whether they contain vegetable growths capable of producing fever, if introduced into the system, is another matter. If it be made out clearly that they cannot disseminate fever germs by the medium of the air, it is a great point gained. And it appears to me as certain that the *contents* of privies never produce typhoid fever through the atmosphere, as that rabies can only originate in the canine race. For if these collections of excreta do not cause fever when they are disturbed by removal and when a larger surface is exposed, and especially among men who have been known to do night work for years; *a fortiori* they cannot cause them when quiescent and

when a relatively insignificant superficies is presented. The explanation of this seems to be that whatever forms of vegetable growth may supply typhoid germs, the conditions supposed preclude their efflorescence and fructification. In any case, even assuming that these plants effect a lodgement on, or come to the surface of, privy contents, and may vegetate up to a certain point; it is clear they cannot proceed to the formation of a perfect mildew. They may possibly start a vibriform vegetation which may spread through the whole mass, as in the case of the yeast plant; and this kind of vegetation may be efficient, when introduced within the human body by any means, in causing fever. But this is extremely problematical. For my part I do not believe in the existence of this form of vegetation—a form capable of inducing fever—in the contents of privies, either on the surface, or in the mass. I have adverted to it as a possibility only, and not as even a reasonable probability. Indeed I am under the impression that the contents of all privies in use, and under ordinary circumstances, remain unaffected by the vegetable organisms which cause enteric fever. When what were contents escape, or are removed, other conditions supervene, which may, and frequently do, cause vegetation of a special kind. But whilst excrementitious matters are confined to the receptacle, I conceive these fungi are not formed. Until they ooze out into the soil, or are taken away, they are, as I think, absolutely void of active living fever germs, So far they are fungus-free. Of course I speak of ordinary privies. I am not so sure about the reservoirs that may receive the excrementitious matters from water-closets; for there the admixture of large quantities of water may introduce another set of conditions; and the possible stoppage of excrement in an angle of a conduit-pipe, or on the valves, or on the lower edge of the basin, or anywhere in fact where its presence may not be detected for some time; may lead to the vegetation of fever germs. Spores and filamentous portions of the plants growing in such positions, may be swept off when the pipe is flushed, and carried into a reservoir where the amount of water may have so far diluted the urinary elements of the excreta, as to permit of the ready growth and spread of the fungi; thus converting the whole of the contents of the receptacle into a material able to cause, and to propagate the cause of, enteric fever. Most persons who have had occasion to enter the water-closets of modern houses will have observed a mouldy smell—not an odour from the combinations of gaseous compounds, such as the sulphurets and carburets of hydrogen, but a peculiar, unmistakeable, musty, smell, evidently due to some form of mildew. The chances are greatly in favour of the supposition that the organic particles floating in the air and affecting the olfactory nerve, and entering the respiratory passages, come from fungi growing on fœcal matter. And though these may not be typhoid germs, or may not be present in sufficient quantities to cause an attack of fever if they are; yet they indicate that moulds or mildews may form under such conditions and may become a source of danger.

178. Again, if a privy be left unused until its fluid contents have evaporated, the residuum of excrement will probably be overspread with mildews of some kind—those of fevers perhaps. With these exceptions and one or two others which, it may be, have not occurred to me, I conclude that the contents of privies, or of excrement receptacles of any kind, do not as such contain the typhoid poison. At the same time it must not be forgotten that they may all be instrumental in producing the poison largely by allowing their contents to escape.

179. Though the evaporation of fluids holding typhoid germs in suspension may possibly cause some of them to be given off into the air, the pollution thus brought about must be slight and hardly sufficient to induce the disease. Yet a very few sporules distributed in this way might suffice to start a mildew growth which, under favouring conditions, and an extensive substratum, might assume very large proportions.

180. It having been argued that the contents of privies, middens, cess-pits, and other receptacles, are incapable of producing the typhoid mildew, or of permitting its aquatic vegetation, probably on account of the nature of the fluids; it remains to treat of fluids and semi-fluids containing fœculent matter without admixture with urine, or other material which would preclude the formation of the typhoid germ, or its water plant. There can be no great doubt that water containing solid fœces, or disintegrated fœcal matter, is always prone to pollution; and, in the event of its being exposed for any length of time in the neighbourhood of the terrestrial mildew, the chances would be greatly in favour of its receiving sporules, or cellular parts, floating in the air, and of its becoming thus polluted. Pollution becomes a certainty when it happens that the aquatic cryptogam is washed, or drained, into a fluid or semi-fluid of this kind by any means. And when once the parasite is established, the probabilities are it will continue to reproduce itself so long as any pabulum on which it can subsist remains. The more immediate point, however, is whether the air can be infected by these semi-fluid collections of fœcal matter; and upon this point a modified conclusion only can be arrived at. That is to say supposing fœcal matter to be completely submerged in an aqueous solution, the surface being liquid, I conclude there will be no direct air-pollution. But if, as may happen, excrement rises to the top of such a fluid and projects, or forms a scum, the upper portion of which is partially or wholly exposed to the air, and becomes more or less solidified in consequence; there is a likelihood that the mildew of typhoid may overspread the surface, and that the atmosphere of the neighbourhood may be infected in a ratio corresponding to the quantity of mildew formed —a thing to be determined by the extent of the mildew field and the more or less favourable conditions for mildew formation.

181. *Excrement-tainted water* as a factor of enteric fever remains to be dealt with. The mode by which it becomes tainted has, however, been already suggested and there is very little more to

add. It is superfluous to allude to the numberless ways by which fluid holding the transformed mildew of typhoid in suspension, may get into drinking water. The only question in immediate relation to the present subject is the length of time this aquatic vegetation will retain its vitality, and its efficiency as a cause of enteric fever, when introduced into water used for drinking purposes, and therefore containing no more fœcal matter than accompanies the typhoid germs. This, however, is a practical question altogether beyond my scope. It is one which can only be determined experimentally, and must be left to those who have the control of effluent waters from the excrement-sewage of large towns.

182. To return to the subject of the disinfectants to be employed in the destruction of the infective matter of enteric fever. Whether the poisonous substance be mildew, or some other product of excrement, I contend there can be no rational, philosophical, or satisfactory, system of disinfection, until that product, whatever it is, is determined. It is an absolute certainty that the poison must come from gaseous compounds, or from animal, or vegetable, sources. For the reasons given I deduce that it is a result of mildew vegetation. But this deduction is arrived at by a process of inductive reasoning which, I am quite prepared to admit, may not only be unsound in itself, but is at best merely a sound hypothesis —seeing that the data are assumed. I advance my propositions, therefore, knowing full well that they may be upset by facts, or by other reasoning. In any case, however, it does not admit of question that the precise cause of enteric fever must be demonstrated before disinfection, as regards that affection, can be placed upon a sure footing. If the mildew view be found wanting and has to be discarded, investigators will have to search deeper; for assuredly there can be no true disinfection where an unknown thing has to be counteracted or destroyed. Rule of thumb will not do here. A clear knowledge of the principles upon which the work has to be done, is indispensable. The interests involved are so large that, sooner or later, the subject must be redeemed from the position in which it now is—a position which must be admitted to be one not far removed from pure empiricism.

183. Let it be assumed that mildew on excrement is the factor of enteric fever. The object of disinfection will then be clear enough—the destruction of the mildew. But the problem how to compass this is yet exceedingly complex, when considered in relation to large populations. It naturally resolves itself into two main propositions: viz., that as to the disinfection of terrestrial mildew, and that as to the disinfection of the aquatic vegetation into which, it has been assumed, mildew is transformed. It is not my intention to enter into details connected with the matter. It is too important and serious a subject to be undertaken lightly and with insufficient materials; and I must therefore leave the various modes of destroying mildews in air and water to those more competent to deal with them. The first thing to be done, undoubtedly, is to settle whether there is such a mildew as I have

suggested; and that, with the means and appliances at hand in Europe, should be no very difficult thing to do. If this hypothetical mildew is not to be found on investigation, the question as to the specific infective substance of enteric fever will be an open one, as it is now. But the scientific world is bound to pursue it unceasingly, until the mystery of causation is laid bare. Until that is accomplished all attempts to disinfect "excrement-sodden earth, excrement-polluted air, and excrement-tainted water," must be crude, tentative and uncertain.

184. If these attempts to disinfect excrement be vague and unsatisfactory, the efforts to deinfect excrement must of course be equally vague and unsatisfactory. The intent of such efforts being to prevent excrement from evolving the infective material *de novo*, and from becoming contaminated by communication with an infected source, I do not see how the end is to be attained in either case, without a knowledge of the infective matter and the conditions of its formation, and evolution. The difficulty would be great in operations of magnitude, even if the precise dangers to be avoided, or overcome, were fully apprehended; but when such enormous schemes of excrement-disposal, as are now entertained in England, are gone into in absolute ignorance of the principles upon which deinfection should be based, it need not be wondered at if, in some instances, there should be a wasteful expenditure, or, what is more important, an utter and disastrous failure. It seems almost impossible but that some miscalculations should take place where unknown quantities are introduced. This has not escaped the notice of those who have been called upon to act—as will be seen presently—still no leading or guiding principle by which to determine when excrement has been deinfected, has been discovered. And indeed it is not known whether or not it can be thoroughly deinfected; and there is doubt, hesitation and mistrust on all sides.

185. If readers are curious upon this subject, I refer them to an interesting Report published in 1871 by the Birmingham Sewage Inquiry Committee. I may say that the committee were appointed to inquire into the sewage of Birmingham, chiefly because the Right Hon. Sir C. B. Adderley had obtained an injunction restraining the corporation from allowing effluent waters from the sewage passing into the river Tame and polluting it. This, and another injunction obtained by the inhabitants of Gravelly Hill, against the nuisance of drying "deposited slush," led to the necessity for some comprehensive plan of dealing with the excreta of the place. The end of the inquiry is that the committee suggest the removal of all the fœcal matter to 800 acres of land, there to be "defœcated," or broken up, and rendered harmless. To illustrate their position, I append an extract from the letter of their consulting engineer, Mr. Thomas Hawksley, C.E., from which some notion may be gained of the coming difficulties of all large towns from the accumulations of excreta. Mr. Hawksley says:—

"There is, however, no instance in which the defœcation of "sewage containing so vast an amount of fœcal matter—the fœcal "matter of 350,000 people—has been as yet attempted, whether by "irrigation or otherwise; nor is it possible from any existing expe- "rience to predicate what may be the pecuniary and sanitary diffi- "culties to which the attempt to accomplish this object might "ultimately lead. Of one important fact we are however assured, "namely, that the strongest legal obligations imposed by the "Legislature and enforced by the Courts of Law and Equity will "necessitate the acquisition from time to time, and at any cost, "of greater areas of land and of larger operations, until the "effluent water shall at length regain from any extreme of pollution "that high standard of purity which, according to modern views, "is necessary to render it legally admissible into any public "stream."

186. When it is added that "the ordinary dry weather discharge "of sewage already amounts to seventeen millions of gallons daily "(Sundays excepted)," an idea may be formed of the extent of the operations to be conducted, in a great measure, in the dark. For although Mr. Hawskley evidently apprehends danger at some future period, neither he, nor any of the authorities quoted in the Report, have any clear perception of its nature; and, in the present state of the question, it requires no prophet to predict some painful results, if not some awful catastrophe, some day. Of course the towns adopting systems of complete excrement-removal will be relieved from enteric fever. That will be a positive gain no doubt. And perhaps the sum of lives lost through fœcal matter may be reduced. But unless persons have some safer guarantee than is now to be had, that the proposed modes of defœcating the excreta of towns are effective as against enteric fever, those who live on streams and depend on them for their drinking water, may be reasonably excused for seeing that the high standard of purity of the effluent water from the fœcal farms above them, which is demanded by modern views, is carefully maintained.

187. If mildew should transpire to be the infective typhoid poison, as I believe it will, and if prompt and effective steps be not taken to guard against its formation at large excrement depôts, it may readily be conceived that the vegetation once started might spread under favouring conditions like wildfire, over an immense surface of ground; and might also lead to an extensive growth of the aquatic plant. I have carefully gone through all the processes of filtration of excrement through soil, and the plans of irrigation, and the methods of farming with excrement manure at Merthyr Tydvil; and I have examined into all the modes of conducting what is called defœcation referred to in the Report; and I am constrained to say that unless more light be thrown upon the dangerous material now being dealt with so loosely and unphilosophically, some frightful pestilence may be the result. I cannot find that the precise cause of typhoid is either known or suspected. It is believed to reside in excrement somewhere, and that is all. It is further supposed that by the thorough disintegration of the

substance, the danger connected with it is averted. And this is true no doubt. But when no one knows the side on which the danger may present itself, how is it possible to guard against it? Who can say that the steps taken to ensure the destruction of the material may not, in some cases, be the means of affording the very conditions for the formation of the infective agent which it is the especial object of defœcation to prevent? I fail to discover in all these schemes a single concrete idea as to the real nature of the work to be done—save and except the destruction of excrement by the most feasible or available means. And this is a clear, tangible and essential thing to aim at, undoubtedly, and must in any case be the ultimate end of the deinfection of excreta. But since no one appears to know what specific poison emanates from excreta, or how it is given off, or under what conditions, it seems not unlikely that some among the many establishments for defœcation may become extensive nurseries for typhoid germs. Nay more than this, if in any of the processes of filtration, and more particularly in some of the farming operations, liquid highly charged with excrement should be poured over the surface of the ground and should be allowed to remain exposed for any length of time, it may form a ready substratum for the cholera germ, or, under very peculiar conditions, it might actually resuscitate epidemic dysentery once more in England. The Chinese produce the latter disease largely, on precisely similar principles to those adopted in one or two instances of excrement farming in England. Given the favouring conditions at some time and there will be the inevitable result. In fine the people of England are steering into a hazy sea of troubles, without chart or landmark. They know the port to make for, but they know nothing of the intervening shoals and sunken rocks; for science has not yet taken soundings. I have no hesitation in saying that all the Courts of Law and Equity, and Privy Councils, and Boards of Health, will not save them from mishap, unless those minute germs which carry death with them are brought into the full light, and their natural history made manifest. There can be no safety for prince, peer, or peasant—as the nation lately saw to its distress and dismay when the Heir Apparent was struck down with the subtle and insidious poison of enteric fever—until investigators have succeeded in tracing out and thoroughly exhausting all the remote as well as proximate causes of the specific diseases which have been under consideration. And here again I would ask if such a subject as this is not a matter of national concern? How are such vast questions as these to be grappled with in private life by private men? Are not typhus and typhoid fevers, diphtheria, small-pox, and the other exanthemata, all and singly, of infinitely greater moment to Great Britain, and Europe generally, than cholera? And why in the name of common sense should cholera alone have its special Conferences? If it be that cholera invasions are so alarming, by reason of the suddenness of the attack, and the rapidity of the course, of the disease, it may be asked whether, from any point of view, it is not more truly alarming for a thousand persons to sink slowly than for a hundred to perish within two days? The political economist, who has nothing to do with individual suffering, but handles the figures of life and death in the abstract as they relate to the general well-being of a community, knows that 100,000 fatal illnesses lasting a month cost a country a relatively larger sum—by depriving it of so much more capital or labour—than if they averaged only a fortnight. It therefore looks somewhat like a weak concession to sensationalism on the part of statesmen, to send delegates to an International Conference on Cholera, whilst at the same time they leave the really more serious diseases every day knocking at their doors, to ordinary measures, or to chance. Let it be understood that I do not assail the principle of the appointment of the Cholera Conference in the slightest degree. On the contrary it appears to me one of the most sensible things in this direction done by the large powers. But standing out as it does, by itself, as a single piece of legislation to provide against one special form of disease, in reality not one-tenth of the importance of any one of half a dozen other forms of disease, it is a practical and bitter satire. For the inconsequence of the thing is so glaring, when examined by the light of a simple arithmetical calculation. Thus—If it be worth a nation's while to expend, say, £5000 in the endeavour to avert 20,000 deaths, from a preventible disease, every ten years; what is it worth expending in the endeavour to ward off the loss of 20,000 persons every year from another, equally preventible, disease? In fact the prominence given to cholera whilst such a terrible scourge as enteric fever is thrust into the background—to say nothing of the other preventible diseases always present in Europe, each one of which carries off annually far more than all the victims of every cholera visitation put together—the exclusive position assigned to this exotic affection, is one of those anomalies which may be ranged under the head of things that no man can be expected to understand.

188. The disinfection and deinfection of sick rooms and hospital wards are vastly important, but the same general principles are applicable there as on the larger fields. The leading idea in every case is first to learn what has to be disinfected, before setting to work to disinfect it. Unfortunately the present state of knowledge is such as to preclude absolute precision in counteracting or destroying infective, or contagious, matters. Yet valuable work has been done nevertheless; and I know of none that has been conceived in a more philosophical spirit than by Dr. Day, of Geelong, in this colony. His investigations of allotropic oxygen led to the discovery of that most delicate test for blood—the guaiacum test—and also to that of ozonic ether as a remedy for diabetes, both of which are now well known and recognised in England. But his more recent experiments with a view to the practical adaptation of allotropic oxygen, especially in the form of peroxide of nitrogen, to the purposes of disinfection, are more interesting still, and of

larger importance to humanity. Dr. Day stated in a lecture he delivered on the 5th July, 1872, in the Technological Museum, Melbourne, speaking of ozonic ether, or etherial solution of peroxide of nitrogen:—"I quite believe that it possesses the power of oxidising and destroying the poison of small-pox, scarlet fever, and typhoid fever." If this be so as regards small-pox alone, and if Dr. Day should happily have succeeded in hitting off a ready means of decomposing, effectively, the organic infective matter in the atmosphere surrounding persons suffering from that loathsome disease, the possibility is presented of stamping small-pox out of the world at some not very distant period, supposing it not to occur *de novo*. But whether this will ever be achieved, or not, it will be a grand result if this terrible exanthem can be prevented from spreading from individual cases, by isolating them with an atmospheric cordon of ozonic ether. I do not know what measure of success Dr. Day has met with in his experiments or whether he has been able to demonstrate his views to his own satisfaction, and to assure himself of their perfect soundness; but there can be no doubt the subject will be treated by him exhaustively. When his conclusions are matured, there will be another great question for nations to deal with. For what can men do single-handed in establishing the truth of their doctrines, or their practical utility, in matters of this kind?

189. To go back to my own thoughts upon disinfection and deinfection. I have indicated the principles upon which I conceive they should be based and have illustrated my views by enteric fever. Putting aside the mildew hypothesis, which may be looked upon by some as a merely speculative notion, I think it must be admitted that no true disinfection, or deinfection, can have place until the specific poison of typhoid is demonstrated. Whatever may be the infective material, all interim efforts to destroy it, or to prevent its formation, must be liable to fall through, whilst it is unknown. If this be granted as regards enteric fever—and the position appears to be unassailable—by a parity of reasoning we are brought to the same conclusion as regards all the other diseases depending on specific poisons. Discoveries may be lighted upon, no doubt, by which some of the unknown poisons may be modified, or utterly destroyed, without a clear insight into the mode by which the end is attained. But no thoroughly sound or enlightened system can be established without a knowledge of the causation of the disease—more especially with reference to deinfection. The disinfection of sick rooms and of the clothes and excreta of patients, may be compassed by agents capable of decomposing all organic material indiscriminately—animal or vegetable—whatever its nature, or its source. An universal disinfectant of this kind would be beyond all price, even though nothing were known of the germs it destroyed. But deinfection stands on a very different footing. For here the object is not to destroy germs already formed, but to prevent their formation; and it is impossible to do this without knowing where

the germs come from; and it may be extremely difficult and hazardous to attempt, if, although the source of the germs is known, their nature and conditions of evolution are unknown. To illustrate the position let diphtheria be taken. Granted that there is an efficient disinfectant of the germs of this disease;—in what direction are we to look to prevent their growth by deinfection? Where is the nidus of the germs? And what are they? Nothing, absolutely nothing, is known of their substratum at present; and it is manifestly futile to try to guard against their ravages by any preventive measures, so long as even the quarter whence the danger comes is unsuspected or undetermined. When the discovery is made it may turn out the simplest thing imaginable to destroy the germ-forming material of diphtheria—just as simple in fact as to prevent the formation of the dysentery poison. And there could not be a more perfect example, by the way, of the value of the knowledge of the source of a specific poison and its conditions of evolution, for purposes of deinfection, than will be afforded by the eventual discovery of the dysentery germ—if my views turn out correct. In that case the means to be adopted to prevent the evolution of the dysentery germ, will present the best illustration possible of deinfectant measures derived purely and solely from, and based entirely upon, a knowledge of the source of the germ and some of the conditions of its evolution. Hitherto deinfection *qua* dysentery, has been as obscure and as impossible to achieve, as that for diphtheria; but to my mind it is now as clear as day. I could wish diphtheria were in as satisfactory a position. As it stands, however, it seems to be hopeless to expect that any community can be either forewarned or forearmed against diphtheria. All deinfection directed against it must of necessity be merely tentative. And the same may be said of that formidable epidemic—influenza. This disease, which spreads over the world at uncertain epochs, and generally numbers more victims than cholera in the countries it invades, is wrapped in just as impenetrable a mystery as diphtheria. Deinfection here also is utterly precluded:—though when a future careful study of its poison germs shall have led to their being traced to their primary substratum, or to the substratum they seize upon in Europe, there may be found no valid reason why civilised nations may not turn aside this pestilence. The Pandemic wave theory of the extension of such diseases as influenza, cholera and plague throughout the world, is quite compatible with the fungoid theory. And I believe it will be shown eventually that these Pandemic waves are neither more nor less than waves of mildew spreading over dirt.

190. To return to enteric fever. I shall have more to say about typhoid when I come to typhus and have to contrast the effects of disinfection and deinfection in the two diseases. I will now pass on to cholera.

ON

DISINFECTION AND DEINFECTION

IN RELATION TO

CHOLERA.

191. The inundation of Europe with another "cholera wave" is just now imminent, and the nations are preparing to receive it. It is advancing steadily and it is a question of a few months only before thousands will be struggling in it. The minds of men are everywhere employed in devising means to break its force and are casting about for such sanitary measures as may afford some prospect of safety. All are trying to solve the great problem of deinfection as against the coming plague. Yet its solution even at this moment, I fear, would avail them but little in their present strait; for few European towns could be deinfected in the short breathing space they will have before the dreaded wave is upon them. I cannot hope, therefore, even if the deductions I have made as to the cause of the disease be thoroughly sound, that they will be of material use in the now threatened invasion. Whatever light they may throw can only have a prospective value. However, there is no saying how soon they may be useful—even though mere inductive reasoning upon assumed data may not carry much weight.

192. But I will nevertheless consider the problem of deinfecting a city for cholera. The causation of this Asiatic plague, which strikes such terror into Europe when it sweeps down by way of either of the two great channels of communication with Hindustan, is neither so obscure as that of diphtheria is, nor so clear as I venture to think that of dysentery soon will be. It occupies a position midway between the two diseases; for the original substratum of the diphtheria germ is altogether unknown, but the choleraic germ is now generally recognised as one having its substratum in organic matter in the soil. Modern investigators have, indeed, nearly unanimously concurred upon this one point, and the old hypothesis of mildewed rice as the starting point of the affection may now be said to be discarded. All the recent authorities trace the infective agent to some form of decomposition having its seat somewhere in the earth's crust; and some of them consider fœcal matter an important, if not an essential, element in the production of the poison:—a conclusion at which they have arrived, apparently, from the fact that the stools of cholera patients contribute largely, if not exclusively, to the propagation of the so-called "cholera wave" from the East to the West. As well as I can learn the

general opinion is not, however, so clear that excrement is the principal factor of the cholera poison, as it is that excrement is the main source of the typhoid poison. Yet putrefaction, or fermentation, or decomposition, of organic material of some kind taking place in the soil, is recognised by nearly all writers as a necessary condition for the production of the ripe cholera poison. I think this is a pretty fair, though brief, exposition of the present state of opinion, setting apart the pollution of water, upon which subject there are many theories not necessary to be gone into here. Assuming then the general opinion to be correct, as to the mode by which the cholera wave is kept rolling through Europe, the problem now to be submitted is this:—To prepare a city for a threatened invasion of cholera, so that those germs of the disease which may be carried thither shall not find a substratum wherein to multiply themselves and propagate the affection: or, in short, to deinfect a city for cholera.

193. It may be stated *in limine*, that I do not propose to myself to endeavour to solve this complex problem. All I shall attempt will be to convey the notions I have formed upon the mode in which it may be possible to solve it; and as to the preliminary steps which must be taken, and the previous questions which must be settled, before there can be the slightest prospect of solving it. In the first place I submit that the same general principles advanced as to the disinfection and deinfection of the typhoid substratum, apply with equal force to the cholera substratum. With slight modifications every argument used in the one case will have to be used in the other case. I do not intend, however, to take readers over the same ground again, but will summarise the conclusions which have been arrived at by the same processes as before.

194. As in enteric fever, so in cholera, the first essential point to determine is the nature of its poison germ. Eliminating details, I conclude it to be a vegetable germ; and, from previously expressed views, it will be gathered that I conceive it to come originally from fœcal matter. I now add that I believe it to be mildew formed on human excrement under unknown conditions in certain districts of India; that this mildew follows an analogous course in its evolution to that of the (hypothetical) mildew of typhoid; that it is transformed into an aquatic form of vegetation by precisely similar means and with the same results; that it probably differs from the typhoid plant in the conditions of its growth, or reproduction, being either more tenacious of life, or spreading with greater rapidity, or being retransformed into the terrestrial mildew from its aquatic state, with greater facility—as would appear from the larger area over which it propagates itself in a given time; that it is maintained on, and acquires its poisonous qualities from, excrement in Europe, though it may overspread other organic matter in the vicinity of excrement, and, as has been supposed in the case of typhoid, the variations in the symptoms may hence be produced; and that the fungoid growths found in the

discharges of cholera patients and named the Zoogl&oelig;a Termo by the Germans, is, most probably, the aquatic form of the tropical terrestrial mildew of cholera.

195. If this view of the hyphothetical mildew of cholera be examined by the light of all the known facts relating to the manifestations of the disease, as it has been observed in every part of the globe, I venture to think it will stand the test. It adapts itself to every peculiarity and apparent anomaly, and fits in equally with cholera on land and cholera on shipboard. There is not one single circumstance in connection either with the origin, or the spread, or the symptoms, of cholera, that does not admit of a full and ready explanation on the assumption of this exotic mildew, and its power of tainting water by transformation from the terrestrial to the aquatic form of vegetation. Moreover all the important, elaborate and ingenious theories extant, concerning the causation and propagation of cholera, may be reconciled, or squared, or where they are defective, or fallacious, their defects and fallacies may be detected and set right, by this hypothetical cryptogam. It supplies the missing link in the chain of reasoning of all writers, and furnishes that unknown thing which is wanting to the completion of the views of most modern authors—from Snow to Pettenkofer. In fact after such reflection and consideration as I have been able to give the subject, I say that, as a matter of induction, the cholera poison must be some fungus, mould, or mildew, or vegetable growth of some kind of cryptogam, either originating on, or deriving its characteristic properties from, fœcal matter. If this mildew be not the actual cause of cholera, I cannot conceive any other possible, or efficient, cause. I may be proved to be wrong by facts; but this is the irresistible conclusion to which I am forced by my present lights, and I therefore contribute it, for what it is worth, to the etiology of cholera.

196. It may be interesting to those who are conversant with the subject, to test the mildew hypothesis by examining it side by side with the most ingenious and beautiful piece of reasoning put forth by Professor Pettenkofer. This will be found at page 493 of the New Sydenham Society's Retrospect for 1869–70. I cannot now delay over the subject, but I must point out the following portions of the Professor's conclusions.

"(3.) The facts of the local and seasonal frequency and spread
" of cholera in India, as well as its extension beyond the frontier,
" warrant, and even demand, the hypothesis, not only of the
" existence of a specific germ or infective matter communicable by
" contagion, but also of an actual determinative local and periodical
" substratum, without which the specific cholera germ cannot
" produce cholera in the human subject, since this disease appears
" to be caused only by a specific product of a fermentation between
" the cholera germ and the cholera substratum. (4.) We may
" indicate the specific germ by the symbol x, the local and seasonal
" substratum by y, and the product generated by these—the pecu-
" liar cholera poison—by z. (5.) Neither x nor y, alone can pro-

“ duce cases of cholera; it is only z that can do this. (6.) The “ specific nature or quality of z will be determined by the specific “ germ x, and the mass or quantity of z by the mass of the sub- “ stratum y. (7.) The nature of x, y, and z is as yet unknown; “ but we may accept it as a scientific probability, bordering on cer- “ tainty, that all three are of an organic nature, and that x at “ least is an organic germ or body,” &c.

197. The mildew view dovetails with this readily. “ The specific “ germ x,” is the exotic mildew. “ The local and seasonal sub- “ stratum y,” is fœcal matter in a certain condition. And “ the “ product generated by these—the peculiar cholera poison—z,” is the original mildew reproduced. Though I cannot go through the whole paper and take each paragraph seriatim, the reader will find that everything it contains may be seen at a glance by the light of mildew. All the facts collected by Pettenkofer, and all the conclusions connected with the oscillation of ground-water, may be read by the view taken as regards the pollution of water by the typhoid germ [173]. They need not be recapitulated; for, *mutato nomine*, the one set of observations apply to both germs. There may be some points of divergence, but the general rules affecting the behaviour of these [and some other] mildews when under water, are probably nearly identical. “ The striking immunity of “ Lyons from cholera,” which Pettenkofer has so successfully shown to depend solely on the height of the Rhone and Sâone, [by which the level of the ground-water in Lyons is governed, and the soil of the city is, therefore, almost invariably saturated, or nearly submerged] is simply to be ascribed to the fact (?) that there is consequently no substratum for the terrestrial cholera mildew;—just as at certain seasons Munich, as has also been demonstrated by the Professor, is relieved from epidemics of enteric fever [174]. Nothing can be clearer than the explanation of these two parallel phenomena, if the mildew hypothesis be admissible.

198. It has been found that when enteric fever has occupation of a place lying in the track of the cholera wave, it holds possession as against cholera. Dr. Lawson, Inspector-General of Hospitals, has written on this subject; but, as I have been unable to procure his work, I do not know precisely how his conclusions are arrived at upon the facts observed, except from a short notice in the same Retrospect above quoted [p. 508]. Speaking of Dr. Lawson's views, it is stated that—“ sporadic cases of cholera have frequently “ been met with a long way within the boundary of the fever field, “ and similar cases of fever within that of cholera, but still the fact “ remains that, though the fever and cholera fields approached “ each other, neither disease took the place of the other until its “ force as an epidemic was broken. This fact sanctions the infer- “ ence that the conditions which generate fever epidemics are not “ only different from those which produce epidemics of cholera, but “ are also incompatible with them; and, further, that sometimes “ the one set of conditions, sometimes the other, exists over a large “ area of the earth's surface, and that the one will give way to the

"other without any marked change in the habits or circumstances "of the population these areas embrace." The exigencies of my present position are such, that I am forced to a diametrically opposite view to the inference that fever and cholera conditions are different. I am impelled to the conclusion, that the conditions which generate the two diseases are so closely allied as to be nearly identical; and that it is this which precludes the one from laying hold where the other has already fastened. It appears to me that it is owing to the great similarity of their conditions that both diseases cannot flourish side by side—unless perhaps they both start fairly together; in which case, probably, the more active Asiatic pestilence would gain a temporary advantage over the slower typhoid plague. From the mildew point, I interpret the fact in this way—Where fever reigns before cholera approaches, the typhoid mildew has taken up so much of the available decomposing organic matter on and in the soil that, when the cholera mildew arrives, it finds no substratum whereon to get a footing. The material for its existence has been appropriated and it must consequently die out—in obedience to a well known law of vegetation. The fever mildew and its water representative being there, and having established themselves by means of the very pabulum that the cholera mildew requires for its support, the later comer cannot thrust the older vegetation from the field and is therefore starved out. The same rule holds good conversely. Priority of occupation determines whether the epidemic shall be of cholera or of enteric fever. And when it is borne in mind that neither of these diseases shuts out dysentery, or is shut out by dysentery, it is an inference that not only does the cholera mildew require the same substratum as the typhoid, but it requires it under very nearly similar conditions; and that the conditions necessary for the evolution of the dysentery germ are essentially different from those required for the production of the other germs. Therefore I conceive it is that a dysentery epidemic is quite compatible with a typhoid or cholera epidemic in the same field.

199. If this view of the exclusion of cholera by typhoid, and the converse, holds good, it furnishes another argument in favour of excrement as the substratum of the cholera germ. For it being generally conceded that excrement is at all events the "principal factor" of typhoid, it follows that it is also the principal factor of cholera. And, indeed, making due allowance for the higher degree of virulence of the cholera poison, there is a striking resemblance between the after effects of cholera and enteric fever—as regards both symptoms and lesions. So strong is the resemblance, in fact, that the sequel of cholera is frequently called the "typhoid stage." This similarity in manifestations and organic changes, which would be obscure on the supposition of a dissimilarity of causation, becomes quite clear on the assumption of germs derived from the same source under somewhat the same conditions.

200. The indications, therefore, for deinfecting a city for cholera, would seem to be as nearly as possible the same as those for dein-

fection for enteric fever. In fact, I strongly suspect that if a city can be successfully cleared of the typhoid poison by complete excrement removal, or by thorough defœcation of its excreta, the cholera wave there will be turned aside. And when it is remembered that one highly civilised country has actually succeeded in excluding both these diseases by the one means for centuries, the probabilities are greatly increased. The Japanese—than whom there is no more enlightened people in all that relates to internal polity—have practically long solved the problem of the deinfection of large cities as against cholera and enteric fever, and dysentery also. The hygienic arrangements of Jeddo, for instance,—a city which contained nearly two millions of inhabitants long before London was as large as Birmingham now is—entirely precluded epidemics of all kinds during the period when Europe was devastated by plagues. And this, be it remembered, in a latitude in which the decomposition of organic matter is effected with greater facility and rapidity. Their coarser and ruder neighbours of China have not attained the art of deinfecting their country; for dysentery and enteric fever are always present among them, and cholera has made fearful ravages; whilst notwithstanding all the intercourse between the ports of Japan and the outer world for many years, cholera has not once struck root there. For the dysentery of the coast of China, Japan is now the recognised sanatorium; and European invalids run over to shake off its chronic but exhausting and generally fatal grip.

201. The exact means by which the Japanese municipal authorities deinfect their cities and the country districts at the same time, are somewhat obscure to my mind; for there is a general, if not close, outward resemblance between the excrement-disposal systems of China and Japan. The difference in the result is probably due, therefore, either to the greater perfection and the more elaborate care in the carrying out of details, or possibly, to nicer calculations in the processes of defœcation and general deinfection. In both countries there is a complete excrement-removal system, and in both all human excreta are applied to the land as manure. The Japanese, however, are incomparably more active, shrewd and cleanly in all their habits than the Chinese. Their internal organisation too, as regards corporate matters, is far higher and more effective. Yeddo is a long way ahead of Pekin, and indeed there is no city in Christendom to be compared in any way to this marvellous place in deodorisation and deinfection. From all one can learn from travellers, and I have questioned many visitors to the city, and one long resident there, it would appear that the executive arrangements by which the streets of Yeddo are kept free from filth and the houses and offices preserved from pollution, are on the most extensive scale. An army of scavengers is employed all night in removing the *débris* and excreta of the day. There are no water-closets, privies, or middens; but a modification of the tub and *tinette* is employed, and in many instances an adaptation of the earth-closet system is in vogue. The scavengers of Yeddo

not only clear away the night soil daily, but the dung of the horses and other animals, offal and kitchen refuse; so that the poorest quarters of the city are kept perpetually sweet. Of course this wholesome state of things could not obtain where the lower orders are not refined to the same extent as in Japan. The Chinese lag behind them in all the details, and do the work of excrement-removal even in a clumsy, slovenly manner, while they display none of that sensitiveness and delicacy in the matter of smells generally which the Japanese have. In fact the worst Chinese towns now are nearly as malodorous as the low haunts of the most highly polished European capitals. I fancy I detect a material difference in the immediate object the Chinese and Japanese have in their excrement-removal plans. The Chinese seem to save and collect their excreta simply for manurial purposes; whereas, if one may judge from the extensive organisation in Japan, the collection of excreta for economical uses is a collateral, and perhaps subsidiary, matter. It looks as though the Japanese design to combine hygiene and comfort with utility, while the Chinese have an eye only to the latter;—a distinction likely to cause an immense difference in the ultimate results. But now comes what I do not profess to comprehend, and that is the mode by which the Japanese deinfect the organic material removed from their cities, so that it does not infect the air, or taint the drinking-water. The fœcal matter from a city five or six times as populous as Birmingham, must be immensely difficult to deinfect, and I cannot understand how it has been done so perfectly as it evidently has been. Earth is probably the only agent employed in the deinfection. And how it has been manipulated on such a gigantic scale, so successfully, is really a thing to be wondered at. That the Japanese should have avoided dysentery in their towns by surface scavenging, I can readily understand; but how they should not only ward it off in the environs of towns, but should also prevent enteric fever from establishing itself there at times, I cannot well make out. For the climatic conditions are as nearly as possible those of China, where enteric fever and remittent (which I suspect to be typhoid grafted on ague) are rife; and excrement seems to be utilised in husbandry there very much in the same way. Writers and others have not observed any marked difference in gardening or agricultural operations in the two countries. The Japanese cover their land with semi-fluid fœcal matter after the manner of the Chinese. And yet there must be some essential variation in the process, seeing that there is such a different product. Possibly the Japanese take the element of time into calculation, and estimate with accuracy the period during which excrement may remain exposed with safety. They may have arrived by practical experience, or by the careful study and observation of their philosophers, at a knowledge of some of the laws affecting human excrement and other organic matter. Ages ago they may have learnt an art of which Europe has just begun to see the importance and is now struggling to attain—the art of deinfection. At all events, whether it has been the outcome of accident, or intuition, or induction, the Japanese have succeeded in making their country by far the most cleanly and the most wholesome of all countries.

202. As I did not undertake to solve the problem of deinfecting a city for cholera, I must leave the hints given towards its solution to fructify. If I had time I could draw the parallel between it and enteric fever much closer. The Crimea furnishes material for illustration as to community of origin. For what but excrement could have been the efficient substratum there? The last visitation of cholera at the Mauritius also, is clearly attributable to the vast substratum field made in the Island by the imported coolies. And wherever cholera has invaded the home of yellow fever, the same peculiarity exists, as regards the inability of the one disease to establish itself in the presence of an epidemic of the other, as has been observed between cholera and enteric fever. Where the black vomit is raging, the Eastern germs cannot find their proper nidus; but when they light on a city fever-free, they seize on it readily; and yellow fever is kept out so long as cholera remains. It has been found in the West Indies that the two diseases may be epidemic in districts closely adjoining, and that they show a tendency to alternate—the one creeping in as the other steals out, and dysentery always hanging on to both. In fact I deduce a similarity of origin for yellow fever, cholera and enteric fever. I might also remark at length upon the great resemblance there is between the general effect produced upon a population subject to an epidemic of cholera, and that experienced during an epidemic of enteric fever. The prevailing diarrhœa and the typhoid type assumed by all other diseases in the neighbourhood, are strikingly analogous in the two cases. The only material difference between them is one of extent, or degree. This again argues a close connection in causation. But I cannot elaborate the subject farther. I will only add that enteric fever is of vastly more importance to Europe to make out and thoroughly understand, than cholera; and that the relative prominence given to the two affections, philosophically considered, is absurd, and common sense demands that it should be reversed.

203. Disinfection in connection with cholera is a wide theme embracing a large range of questions:—from the destruction of the fungus in the vomited matters and dejections of individual patients, to the demolition of the germs flourishing on the immense cholera fields of India. Happily for mankind the risk from contagion is of the very slightest. Experience has shown that those brought closely into personal contact with great numbers of the cholera-stricken in hospitals and elsewhere, do not take the disease. The work of disinfection as regards the sick, therefore, is fortunately narrowed down to the visible matters which come from them. The stools and vomits are the points to be attacked. These ejected things contain the *Zooglœa Termo*—the hypothetical water-plant of the hypothetical cholera mildew. Whatever

will destroy the vitality of these fungoid germs, then, is disinfectant; and nothing short of crushing the life completely out of them will be efficient disinfection. This assumed cryptogam exhibits a marvellous fecundity and an extraordinary rapidity of growth and spread. So singularly swift, indeed, must have been its rate of increase and extension in some recorded instances, that I should hesitate to believe in the possibility of a mildew being the efficient cause of cholera, were it not for the frequently observed suddenness with which a cloud of mildew will fall upon a district in some peculiar states of the atmosphere at certain seasons. In what are called exceptional seasons, a few hours will suffice to produce a crop of parasitical vegetation over an area of thousands of acres. These seasons are the epidemical seasons. And an epidemical season must be assumed for the propagation of the cholera germs and the influenza germs and the great Pandemic waves generally, in the direction of Europe. Otherwise instead of a cholera invasion of the western world every few years, there would be an annual inroad; for communication with the endemic centres is probably just as constant and as great one year as another. It follows that a cholera wave only gathers at exceptional times, when the seasonal conditions are favourable to its rise and progress. At such epidemical periods, therefore, I can conceive that a light feathery mould, or mildew, once fairly started on excrement, or other organic matter, might fly over all other substrata for a great distance, with electric-like rapidity. The celerity of extension of the disease, in fine, does not appear to me to exclude the hypothesis of a mildew as the efficient cause of cholera.

204. Assuming the water-plant of cholera in the egesta, I need not stop to discuss whether the ordinary disinfectants and antiseptics used are effective agents in the work of demolition. The question whether zinc, or iron, or copper, or chlorine, or other chemicals bring about complete disorganisation of the fungus and deprive it of its power of fructification, is one of detail, which I may leave—merely observing that I have a strong suspicion that, as usually employed, they do not serve the purpose in all cases. I throw out the suggestion that the application of heat may be made practically available for disinfective purposes. There are three things to be especially guarded against in dealing with the vomited matters and the dejections of cholera patients. 1. The reconversion of the aquatic fungus they contain into a terrestrial mildew. This would seem to be effected so readily that, whatever steps may be found necessary to hinder the transformation, will require to be taken without delay, or the air may be infected with a new set of sporules to start vegetation afresh on the nearest substratum. 2. The admixture of the cholera stools and vomits with fluids containing organic matters capable of sustaining the water plant. Thus the throwing them into gutters, drains, or sewers, becomes a most potent means of propagation, by leading both to the empoisonment of air, and in many instances to the

pollution of drinking water. Where any of these drains, thus contaminated with the aquatic cholera germs, have communication with wells, or reservoirs, the consequence may be eminently disastrous, as was seen in the historical case so well worked out by Snow, in which occurred the wholesale poisoning of those persons in London who got their water-supply from a certain Company. 3. Another great source of danger in the egesta of cholera patients lies in the drying up of the matters; by which means the germs may be preserved and may become active instruments in the further spread of the disease. When pulverised so as to float in air, they may act as a direct poison on those inhaling them, or they may be carried to a favourable substratum for their development and increase. All disinfection in hospital wards must, to be efficacious, be directed specially to these three sources of danger from the germs, and no more likely way of destroying them utterly occurs to me than that of boiling them at once. If, instead of merely adding some disinfectant solution to such excreta as may be received into vessels, before committing them to the common cloaca, these vomited and dejected matters were simply boiled previously to being thrown away, very few germs probably would escape in a condition to fructify. The addition of Sulphate of Zinc, or some other cheap corrosive material, might give additional security. Of course the same principle applies to the blankets and coverlids used. Once a day they might be passed through two or three vessels of boiling water—the first containing some weak caustic alkali. Whether all this is practicable or not, I must leave. It could only be carried out in large establishments where there are means and appliances at hand. But it is a sudden thought which I cannot now work out. It may not even be new for what I know. In any case, however, if the work of disinfection in cholera be worth doing at all, it is worth doing thoroughly. And assuming the *Zooglœa Termo*, or an aquatic fungus of some description, the whole object and intent, end and aim, of disinfection of the sick room, must clearly be to deprive that vegetation of vitality with the least possible delay.

205. The disinfection of European towns, in their present excrement-sodden condition, is a hopeless matter. Until communities apprehend the full danger of their own excreta and take measures to guard against it—until, in fact, their towns and cities are brought into the position in which Yeddo is described to be—they must calculate on periodical epidemic cholera as a certainty from which there is no escape. And when the waves come, all effective disinfection on a large scale will be precluded. The only possible chance of safety lies in deinfection. Fortunately the cholera germ comes of an exotic mildew—a tender plant requiring peculiar seasonal conditions to enable it to maintain its existence in Europe—or anywhere else, indeed, except in its own habitat. Even in tropical countries it soon dies out—there being a marked distinction between the cholera germ and that of yellow fever in this regard. The latter clings to the new soil to which it may be

transplanted; the former flourishes for a while and then suddenly perishes. In cases where the climatic conditions are as closely as possible analogous to those of Hindustan, it would seem that the fugacious cholera mildew rapidly exhausts all the specific pabulum it requires for its support and reproduction. It appears to exact special factors that are not to be found to any large extent out of the regions in which cholera is endemic. Whatever the explanation, however, the fact remains that cholera cannot be naturalised in any country. Therefore it happens that cholera-stricken places are always disinfected, and sometimes rapidly, while allowing things to take their own course. The winter epidemics of St. Petersburgh by the way, so ingeniously explained by Pettenkofer, appear to me to admit of being accounted for in a simpler way by the mildew hypothesis. I suggest that the water-germs when the water is frozen do not lose their vitality. Packed in ice underground they are probably in a living state; and when the ice containing them is melted and converted into potable water they are perfectly efficient infective agents to produce cholera. It is also possible to conceive that stove-heated houses might under peculiar circumstances furnish sufficient artificial heat to force the aquatic germs into an evanescent terrestrial mildew, and thus add a limited amount of air-poisoning to swell the sum of the cases. The same warmth that restored the germs to activity, might suffice to prepare a substratum for their reception.

206. Passing over the stages by which cholera advances towards Europe, and leaving the numberless questions connected with the mode by which it is passed on from place to place, all of which questions may be answered as I think by the light of mildew with its water-plant, I come to the consideration of the disinfection and deinfection of India—the birth-place and abiding-place of cholera. It is a matter of such magnitude, however, as to be a special study of itself. As I can do no more than add a few pages to the huge piles of evidence accumulated on this one division of the great subject of hygiene, I must look at it from my own point of view only. It was my original intention to have taken this question in connection with dysentery; but it may as well come here. Excrement disposal is the basis of the theme, and what applies to the one disease may easily stand for the other. In the first place it may be remarked that there can be no reasonable doubt that cholera is endemic in Bengal. Whether it is, or is not, also endemic in the other Presidencies may be a debateable point; but it is now unquestioned that the sole source and origin of cholera is in British India. It takes its rise somewhere within the four corners of Hindustan. As then it is not endemic in any other country, it follows that if it could be stamped out of Hindustan, Europe would be no longer subject to the epidemic; and, further, if it cannot be stamped out of Hindustan, and if the intervening regions between Hindustan and Europe be not deinfected, and if Europe itself be not deinfected, it follows that Europe must always be subject to epidemics of cholera; and, by way of Europe,

the rest of the world will be liable to be infected as it has been heretofore. If this statement of the position be accepted, the object is plainly to disinfect and deinfect India; or to deinfect the intermediate countries; or to deinfect Europe; or, lastly, to deinfect the rest of the world. Practically I suspect that for some indefinite period there will be no help for Europe from without. However let the question as regards India be examined.

207. The point is whether India can, or can not, be disinfected and deinfected, say, within fifty years, and endemic cholera be thereby stamped out. All things considered, I incline to the opinion that the British will not succeed in disinfecting and deinfecting India and exterminating cholera within that time—even though the causation of the disease be clearly established in the next two or three years [unless it should prove to be other than the specific fungus I take it to be.] Assuming that a mildew developed on excrement under certain conditions be determined, by irrefragable proof, to be the cause of endemic cholera, I believe it would even then be absolutely impossible for many generations to come to prevent the development of the mildew on excrement by modifying, subverting, or suppressing, the conditions. If this hypothetical fungus be brought home at last to excrement, it will require a great amount of education or legislation, to overcome the bigotry, or the inertia of the Hindoo, or the Indian Mussulman, so far as to induce him to change a custom hereditarily connected in his mind with abstract purity. Of the 180 millions of people living within our East Indian territory, it may be computed that 150 millions void their solid excreta in the open air and leave them on the surface of the soil. Some thousands of tons of excrement have thus been daily deposited on the earth for some thousands of years. The area over which this mass of material has been spread is undoubtedly an immense one; yet it must not be overlooked that it has not been uniformily spread, but has been mainly concentrated round the centres of population. Outside the larger cities of India, the bulk of the people are to be found in villages of from two to ten thousand inhabitants, and from five or less to twenty or more miles apart, according to the natural features and fertility of the district. Surrounded with their cactus hedges many of these villages are probably much in a similar condition now, to that in which they were in the days of the oldest nations of which we have any knowledge. To most of them very few individuals have ever migrated; and the residents have not only been born there, but they have never gone so far as the next village. They have cultivated the same paddy fields, or the same patches of grain, for countless generations. Every morning at the first dawn of day the whole village issues forth, chatties in hand, each human being passing out by the same place of egress, on the same point of the circumference, to the same spot of ground on which he has fulfilled certain bodily functions with certain peculiar rites from childhood. The result has been the accumulation, at certain spots in close vicinity to the villages, of enormous amounts of

fœcal matter, and a corresponding degree of saturation of the soil; with its consequence—extensive pollution of water in every direction. But the special object of this description is to impress upon such European hygienists as may not have taken this element of difficulty into calculation, the nature of the people with whom it is proposed to deal on so gigantic a scale. When it comes to be a question of moving scores of millions of an inert population living in this way, surrounded by the strong barrier of caste, it will be seen that the introduction of efficient sanitary measures must be painfully slow work. If now we turn from these villages containing the lowest, or least enlightened, classes, to the large cities of India and to the people clustered along the banks of the great rivers, we shall find not only the same peculiar habit to which allusion has been made, but even more serious evils necessarily accompanying it. For of course the more concentration the more mischief. Possibly, however, these people, from rubbing shoulders with the world, may be more plastic and more amenable to change. It has been seen that the holy doctrine of Sutteeism has been considerably modified—at all events as regards the outward observance of the practice—and it may be that a radical alteration in other forms of procedure may be possible in time. But whatever may be accomplished with the more civilised, and therefore more ductile and impressionable, natives of India, it must be a prodigious effort which shall revolutionise the stolid, narrow-minded, calmly patient, tenacious, superstitious, millions, congregated in the interior of this vast country.

208. The present rulers of India are not likely to bring about such sanitary reform in Indian villages as would extinguish cholera. There is only one way of dealing with Orientals; and that way the British people will not take. Besides, it may be fairly doubted whether even a despot, endowed with rudimentary hygienic instincts, could settle this matter summarily and clear Hindustan of cholera in a year or two. If a real, absolute, tyrannical, potentate of the ancient type, swayed the whole country, and by some odd freak conceived the design of keeping it sweet, he could hardly compass his object. The probabilities are he would be deposed, or strangled, or confined as a dangerous madman. But if he escaped, he would find his cruel decrees defied and set at nought in every direction. Long cherished custom, solemn ordinances, religious rites, old prescription, and the natural rights of man, would obstruct his efforts on all sides. Martyrs there would be of course, ready to give up their lives rather than the control of their bodies. Some would raise their voices and cry aloud. Fine obstinate old Hindoo puritans would call on the people to submit to death, but not to ignominy or pollution. And they would submit, with meekness and placidity, to wholesale massacre—not so much on account of being exhorted to submit, as because death in this form presents itself to the Oriental mind as one of the natural terminations of existence. It is this calm contempt of death which would interpose for a long time an almost

insurmountable obstacle to the tyrant's will. Yet the extermination of a few villages and steady, determined, persecution might ultimately have their effect. The Hindoo might be moulded by such pressure into another shape at last. But what prospect is there that the British can bring about a stupendous national change of this kind within a few decades? For the position is worse now than it would have been in Tippoo Saib's time. The Indian mind has undergone a complete change, though the laws of caste are the same. We hold India by a different tenure to that of their Mahommedan conquerors. They governed by might, and struck such terror that the weaker races yielded submissively. We have a mixed sort of policy and try to make might square somehow with right. We have fixed our teeth in India; and, practically, of course we must cling, though theoretically there's a hitch—unless the law of natural selection and development sets it all right. But we have got India; and we have given the natives the notion that they have some liberty of action within certain limits. They find themselves, indeed, very curiously placed for Orientals, and are uncertain what to make of a rule under which there is such singular relaxation, that they are allowed to do pretty nearly as they like, and are not at the absolute mercy, or subject to the grinding exactions, of their local petty despots. Impressed with the idea that man was formed for violence and plunder according to his opportunities, and that it is in accordance with the fitness of things that he shall take who can; but finding himself in a world where the ruling powers leave him his crops and do not swoop down on his hoards of treasure, the Indian is uneasy. The anomaly disturbs his mind. Is he fattening for future sacrifice? Or is this milk and water sway a confession of inherent weakness? And if so, may not the tables be turned? His subtle wily nature prompts him to seek every occasion to revolt. He is always crouched for the spring—and with what marvellous swiftness and malignant fury this usually timid, cringing, docile creature launches himself forth, when he sees his opportunity, England has had proof. Any attempt at coercive sanitary measures in India, therefore; any energetic legislative enactments having the effect of interfering directly with bodily functions, in their mode of fulfilling which the natives can see nothing objectionable, would not only create a wide-spread feeling of the injustice of the thing, but would probably arouse a simultaneous spirit of resistance, which might cost us more lives to put down than several cholera waves in Europe would entail.

209. The disinfection and deinfection of India, then, to an extent to preclude endemic cholera, may under any circumstances be regarded as a remote possibility. Education may do something towards it; and some of the wealthier Hindoos—more especially those who have studied medicine—may assist in getting in the small end of the wedge. But even under the most favourable circumstances—even supposing there was no opposition to be anticipated on the part of the natives themselves, beyond that

mere passive resistance to great social changes which is to be found in all communities alike—the organisation necessary to ensure the practical working of effective hygienic machinery would take some time to mature. Therefore Europe must make up its mind to be swept over by the cholera wave at intervals, for the next half century. But there is an apect of the question of great importance to European residents in India and to the British people generally. Are the sanitary measures already introduced into those parts of India occupied exclusively, or principally, by the British themselves, as thorough as they may, or should, be? And are the precautions taken in moving troops from one part of India to another, rightly taken? No one who examines into this subject and looks through the careful, painstaking, elaborate instructions that have issued from time to time upon all the matters involved, but must be struck with the enormous amount of work done by the authorities. No details, however minute and seemingly trivial, have been omitted. The arrangements connected with the outside and inside of barracks, with the latrines, the stables, and with the whole economy of encampments in fact, are admirable as regards internal cleanliness. And as regards the care taken of men on the line of march, there has been clearly no lack of endeavour to protect them from disease in all shapes. Yet on the hypothesis that many of the deadly maladies of India are caused by a vegetable parasite on human excrement, it will be seen that a vast deal may yet be done in disinfection and deinfection, so far as the British in India are concerned. If I could be as sure that my hypothesis as to the causation of cholera, and other diseases, was as sound a piece of induction, as I am clear that my theory as to the causation of dysentery is, I should have no hesitation in saying that from three to four fifths of the deaths of British residents in India from these diseases, might be prevented.

210. In order to place the present position of the Indian sanitary question with reference to cholera before the reader, I append a leading article from the *Times* of September 23, 1872. As it is a neat exposition and epitome of modern scientific views upon the subject and, moreover, illustrates in a clear and succinct manner some of the ideas I have broached, I give the article in its entirety.

"The recent outbreaks of cholera among European troops in India, to which we have directed attention, have called forth remarks from several correspondents who are more or less acquainted with the local conditions of the stations in which the disease has appeared. Their letters assign various causes and suggest various remedies for the periodical recurrence of the most formidable and most demoralizing of epidemics. In the country itself an Army Sanitary Commission has long been at work upon the question; at present with the sole result that its members have been overwhelmed by the number and variety of the views and statements submitted to them, and that they have been unable to educe even the form of order from such a chaos of material. The fact is that the problems relating to the origin and the diffusion of cholera present themselves to the inquirer in India in forms absolutely too complicated for solution, and which seem to tend only to the production of bewilderment. Where the poison of the disease, whatever its nature, is so widely scattered and so frequently active, it becomes impossible to determine with even an approach to accuracy the share of any single condition in the production of the general result. When Mr. Simon and Professor Parkes announced, as the result of investigations carried on under more favourable because less complex conditions, that contaminated drinking water would furnish a sufficient explanation of the spread of cholera, they reduced the whole question to a more simple form than any in which it had previously appeared. If the value and importance of their work had been recognized in India, steps would immediately have been taken to guard the water supply of military stations from pollution, and thus to exclude at least one element of danger. It is not probable that by this means cholera would have been entirely kept at bay, but its prevalence would have been diminished in the precise degree in which water is the channel of conveyance for its contagion. To this extent the ground would have been cleared for further inquiry, and other channels might in like manner have been discovered and stopped singly, and in succession, until, in course of time, almost complete security might have been obtained. Instead of following this reasonable course, Indian authorities have wholly misapprehended the real bearings of the question. They seem to believe that English inquirers have represented 'impure' water to be a cause of cholera, or even to be the only cause; and Dr. Muir, in the recently published 12th Volume of the Reports of the Army Medical Department, speaks of the question of Indian water supply as having been 'mixed up with theories of cholera and other epidemic diseases.' Dr. Muir and other observers in India often use the word 'theory' as if it were equivalent to 'hypothesis;' and they manifestly fail to apprehend what it is which has been established here at home. The proposition is, not that 'impure' water is a cause of cholera, but that water contaminated by choleraic discharges will reproduce cholera in those who drink it; and this is no mere hypothesis, but a truth resting upon evidence which cannot be contravened. The distinction is of such grave practical bearing that it is worth while to give a typical illustration of the facts on which it is based.

"In June, 1865, Mr. and Mrs. Groombridge, residing in a solitary house at Theydon Bois, near Epping, sought medical advice on account of chronic ailments from which they and some members of their family had been suffering. The practitioner whom they consulted suspected that their symptoms might be due to contaminated water, and desired that a sample of that in use might be sent to him for analysis. He found it to be of unpleasant odour and nauseous taste, containing traces of sulphuretted hydrogen and much organic matter. The event showed that the waste pipe of the sink leaked into the well, and that the water was thus rendered at once unpalatable and unwholesome; but Mr. Groombridge was content with it, and suffered matters to remain as they were. Shortly afterwards he was recommended to leave home for a time, and he and his wife visited Weymouth. Either there, or at Southampton on his return, Mr. Groombridge was exposed to choleraic contagion, but nevertheless reached his home, where he recovered from the first attack. This attack was, however, the means of conveying the special choleraic contamination into the well, with the result that eight persons out of twelve living on the premises were taken ill, and that five of them, including both Mr. and Mrs. Groombridge, died. Four visitors to the house—one of them being the medical attendant, who is known to have tasted the water—were attacked in the same manner, and all of them died, the symptoms in every case being those of Asiatic Cholera in its most declared form. A subsequent inquiry revealed direct leakage into the well not only from the waste pipe of the sink, but also from a soil pipe, seldom used, but by which, as it happened, contagious matters were known to have been cast away. Now, this case, which is supported by many others of like kind, but which, even if it stood alone, would be absolutely conclusive, presents the facts in the smallest possible compass. It shows that simply impure or dirty water may be more or less unpleasant or unwholesome, according to the nature of the dirt or the impurity, but that it would not therefore be a cause of any defined or specific disease. When, however, the poison of cholera, or of typhoid fever, or perhaps of some other malady, is added to the water, whether this was previously pure or impure, then, and then only, is the specific malady reproduced among those who drink it. The obvious corollary is that water for human consumption should not only be obtained from the best accessible source, but that it should be secured on its way to the consumer against the possibility of receiving contamination from sick persons. The necessary security can only be afforded by the use of closed pipes as the channels of conveyance, and wherever closed pipes are not employed the watercourse is liable to become a highway for the current contagions of the locality.

"It is this simple proposition which many Indian Health Officers seem unable to understand, or which they neglect, while they are writing about 'theories' and compiling voluminous Reports. Dr. Muir, in the document to which we have already referred, states that the water for the troops at Meean Meer is obtained from an adjoining canal, and proceeds to declare that much has been accomplished of late to increase the purity of the supply. He admits that a good deal still remains to be done, 'such as the covering in of wells, the provision of pumps, the substitution of suitable vessels for the objectionable mussacks, and improvements in the mode of distribution;' but he appears to regard all these as comparatively unimportant matters of detail, and not to see that, with the habits and customs of the natives, the open canal, the open well, and the filthy dipping vessel are so many totally unguarded inlets through which cholera, or any other epidemic disease, may at any time enter and be diffused abroad among the garrison. Mr. Commissioner Cornish, in his admirable Report on the outbreak of cholera among the 18th Hussars at Secunderabad, says:—'The wells are all open mouthed and unprovided with the simplest appliances for raising water. They are crowded all day long by people from the neighbouring bazaar, who bring their own ropes and chatties to draw and carry water. The chatties are constantly washed at the well, and any foul matter adhering to the outside of them would be pretty certain to find its way into the well waters. It is quite possible that chatties from an infected house, soiled with cholera matter, might have been washed at the wells.' To this account it might be added that several of the wells are approached by flights of steps which lead down below the surface of the water, and that those who go to dip constantly immerse their feet and legs in doing so. In all these wells the need for protection from choleraic discharges may be said to be totally neglected in practice; and it is further asserted that in more than one instance foul water has been strained through filters made from sand which had itself been exposed to choleraic contagion. Such are the conditions against which we have to guard in the East; and they render the quality of the original source of water supply a matter of less importance than the intercepting of accidental pollution between the source and the consumer. Water which was hard, or turbid, or brackish, or in other respects of bad quality, might, indeed, be unwholesome, but it would not be a cause of cholera; and no degree of original purity or excellence would avail if the supposed good water had been contaminated by cholera matter on its way. The protection against cholera which may be gained from closed channels and reservoirs is not, therefore, in any sense a matter of hypothesis. It is a matter of fact, like the protection against rain which is afforded by an umbrella. Like that protection, also, it is possibly incomplete, and may leave some channels of access unguarded. But we do not discard umbrellas on the ground that they fail to keep the feet dry; and we have no right to neglect precautions which would be to a great extent effectual on the ground that further precautions might be required in addition to them. So long as the Indian authorities leave wells and watercourses exposed to innumerable chances of pollution, so long will they retain one efficient channel of cholera in their midst. We speak especially of India, but the truths which apply to India apply equally at home, and will explain many outbreaks of disease in England which would otherwise be inexplicable. The matter is of the simplest kind; and it only needs to be fully understood for public opinion to insist upon improvements which might save tens of thousands of lives."

211. The first thought that occurred to me after reading the above was whether, among the views and statements apparently consigned to the waste-paper basket of the Army Sanitary Commission, any of the writers has hit off the same hypothesis with myself. I cannot help it if he has. There would be one comfort

for me if I cared about priority in such a matter; and that is that my views will see the light before the Commission are able "to "educe even the form of order from such a chaos of material." This reminds me that the cryptogamic view of the cause of cholera is by no means new. Many writers have suggested that the infective material of cholera was the product of a fungus, and have supported the view with great ability. Yet I cannot find that any one has reduced the thing to one plain issue—that of a fungus originating on human excrement. I need not quote the arguments adduced in favour of the fungus view, but I claim them in support of my hypothesis. For the general inductive processes which lead to the conclusion that the cholera infection comes from some unknown form of cryptogam, developed on an unknown substratum, may be employed in the hypothesis that it comes from some one of a given tribe of cryptogams, developed on a specified substratum. If the reasons are cogent in the one case, they may be no less cogent in the other. And in the present instance I venture to submit that they add material weight to all the other arguments in favour of the mildew hypothesis of causation. But to the review of the *Times* on the present posture of sanitary affairs in India.

212. There could not be a better commentary upon the leading principles of disinfection herein suggested, [155 to 163] than is to be found in the above article. It exemplifies in the most perfect manner the confusion, misdirection, and uncertainty, that must prevail in all hygienic matters, so long as the causation of infectious diseases is unknown—so long in fact as the Art of Hygiene is not raised to the rank of a Science. The *Times* is undoubtedly right in insisting on the fact that water is a potent means of cholera infection, as demonstrated by Mr. Simon and Professor Parkes; and the case in point showing how it may become polluted with choleraic discharges, and may thus become the medium of infection, is conclusive. There can be no doubt, either, that the water-supply of military stations in India is here and there contaminated in some one or more of the innumerable ways indicated by Mr. Commissioner Cornish, and by many others before him. It may be admitted also, as an abstract proposition, that since closed pipes from an undefiled reservoir would ensure immunity to a station against cholera from water pollution, every effort should be made —on the umbrella principle, or on another more homely one, that half a loaf is better than no bread—to close up all the avenues by which infective material may find its way into the channels of supply. But how far a closed water-system, pure from its source to its distribution, has been, or is, a practicable measure with the means at the disposal of former or present army medical officers at all the military stations throughout India, is a matter upon which I have no precise data to form an opinion. Judging, however, from the universal *non*-excrement-removal practice of the natives, I should be disposed to think that the principal difficulty in many instances has lain in getting an uncontaminated tank, well, or

enough has been said to indicate what is the principal agent in its pollution, and what means are to be taken to prevent the introduction of infective matter. Details are superfluous. I leave Calcutta, therefore, and go to the interior; but before doing so I would observe that I can see no reason, with my present lights, why that city should not be made as healthy as Port Louis was before the introduction of coolies into the Mauritius, or as Yeddo has been for some centuries past. If the Bengalese and English could be replaced by the Japanese, the conversion of Calcutta into a salubrious city would, I conceive, be the work of a very short time. It will be seen that I do not believe in the inherent or indigenous unhealthiness of climate. And excluding those maladies which are incidental to purely vegetable miasms, and which are confined to certain areas in the tropics and limited to small paludal regions in temperate latitudes, I conclude that the whole earth would be perfectly wholesome, and certainly free from all infectious disorders, but for the errors of mankind. None of the atmospherical conditions, or the telluric, or electrical, or any other natural conditions, in any form of combination, will produce a specific infective germ. They may mould a race and induce physical change of structure; but provided men live in conformity with the laws of nutrition, there is no portion of the earth's surface, on which men can live at all, where they may not live healthily—excepting the marshes and swamps. Of course climate has its effect in propagating disease. It supplies more or less favourable conditions for its spread. And this it is which has led to confused notions and to the general belief that most tropical countries are necessarily unhealthy. But to say nothing of causation, it is now recognised that each specific infective substance must have its specific substratum, or the specific disease produced, disappears—as glanders and dysentery have disappeared from England. And the proposition is that natural forces, conditions, or momenta, in any part of the world, will not supply the substratum of any one of all the specific infectious diseases of man. If this be conceded, it follows that the climate where these specific diseases are rife has not been concerned in the production of their substrata. Therefore man provides these substrata; therefore man makes the atmosphere locally unhealthy; and therefore man has only to cease to provide the substrata, for the local atmosphere to be restored to its original natural condition. The aim of the hygienist is to determine the substrata formed by the agency of man.

217. As the disinfection and deinfection of all the other cities of British India in which the British are compelled to live, must be considered from precisely the same point of view as in the case of Calcutta, there is no occasion to refer to them specially. The vast difference of race in the various parts of the great Empire, will involve many modifications in any general scheme of hygiene designed to preserve the British forces. Many difficulties will present themselves, no doubt, and some serious complications may

arise from enforcing sanitary regulations. But whatever the difficulties and complications may be, there can be no exemption from cholera, dysentery and fever in any of these cities, so long as the one great substratum of these diseases is distributed over the surface of the soil, either in the cities themselves, or immediately outside their walls. I foresee that the question of maintaining garrisons in such positions in an effective state will ultimately resolve itself into one of expediency or cost.

218. The most serious *foci* of disease in India, however, are the villages. And this not from their size, or because the villagers are essentially more foul in their customs than the remainder of the population; but simply because of their numbers and the less plastic and more ignorant condition of these inland tribes. Thousands of such villages scattered over the face of Hindustan, not only provide a wide substratum field for the propagation of endemics and epidemics by the air, but they cause the pollution of almost every water-course, tank, well and reservoir, from one end of the country to the other. There is no safety at certain seasons in the interior anywhere, either in air or water. When the rains come, they not only produce the dysentery germ crop, but they wash it, with the necessary pabulum for its support, into the nearest water. And so with all the other germs. Now the notion of attempting to obviate all this mischief, by any means, appears too wild and visionary. An Anglo-Indian would laugh it to scorn. For the present at any rate there is nothing effective in the way of hygiene to be done with these villages. Practically they must be left to breed cholera, and the "black death," and the malignant ulcers and boils, and all their plagues, as heretofore. And practically we must make the best we can of it. The question is how to counteract the evils we cannot prevent. The problem is this. In the country described certain garrisons have to be kept, troops have to be moved from one point to another and must in many instances be marched through infected districts. How is this to be done with the least risk to health and life? [Postulate of the cryptogamic hypothesis, of course understood]. First as to the cantonments, or military stations. Here the dangers are internal and external, and air and water poisoning may be common to both. The internal arrangements, I perceive, are such as extreme care and anxiety for the health of the cantonment would suggest to enlightened men—in the present state of knowledge in hygiene. Every particle of organic refuse matter is removed from the quarters and the ventilation, comfort, cleanliness and nutrition, of the troops, are zealously watched. So far so good. But dysentery and cholera and fever yet get into these stations and spread and entail great loss of life. How is this? The explanation is simple enough. Setting aside water pollution from without, for the present, I think I perceive quite sufficient in the internal economy to account for the periodical recurrence of these diseases in the cantonment; and after all that has been said it will not be difficult to divine that I allude to the latrinal arrangements. These may be decent and cleanly but yet

highly causative of disease, or active means of its propagation, or both. In fact when Indian hygienists at last realise the peculiarly dangerous nature of excrement, they will also realise the fact that they have stopped short in their sanitary measures just at the very place where decided measures were absolutely essential; and that by omitting to take those measures they rendered null and void many of the excellent measures they had taken. I am perfectly well aware that a great deal has been done of late years in this direction; and that not only more decency and cleanliness has been secured, by selecting better sites for latrines and by enclosing them more and by confining their contents more carefully to the pits or receptacles; but that much disease has been thereby prevented, as the returns of the deaths at all the stations show. But the argument now is, not that a great deal has not been done, and done with a successful—I may say a glorious—result, but that a great deal remains to be done. And the very fact of the successful result gives strength to the argument. For it is admitted that the result is attributable to the improvement in the modern excrement-disposal system. If, therefore, such a result is due to a partial and an incomplete system, what result may not be reasonably expected from a thorough system? I say that the mortality will always be precisely in proportion to the effectiveness of the system, and that when the system shall have arrived at thoroughness, none of the diseases in question can possibly occur endemically; and if any of them be brought into the cantonment by infected men, they cannot spread epidemically—always supposing the subsequent disinfection to be as thorough as the previous deinfection. In fact it comes to this:—that the whole plan of Indian barrack hygiene will have to be modified, or enlarged, to meet the requirements in connection with the deinfection of excrement. Not only will the excreta have to be removed to a certain distance from the quarters, but adequate provision will have to be made even then for their defœcation, or destruction. Far more elaborate processes will have to be introduced and far greater care taken in this matter than are now contemplated. The great questions of excrement-removal and defœcation at this moment occupying the minds of the Sewage Commission of Birmingham and, indeed, of the Municipal authorities of all the large towns of England, will have to be gone into by the Army Sanitary Commission of India. The same knotty points will occur in the two countries, and in all other countries, as to what constitutes complete defœcation in the first place, and as to the safest, cheapest, and most practical way of arriving at it, in the second place. It will be no easy matter to initiate a scheme such as is here indicated, and to perfect it in its working details so that it shall apply to all the varying features and conditions of the present military stations throughout India. It will be a complex problem for some one. But it will have to be solved. England will have the thing done; and those who will have to do it had better look to it in time.

219. The future considerations as to the internal deinfection of military stations, though serious and weighty, will be of secondary importance to those as to the external precautionary measures. The dangers from without come from the extensive mildew poison fields cultivated by the native population, emanating from which swarms of aërial germs bear down upon the cantonment, while copious supplies of aquatic germs are poured into the potable waters depended on for the force. These evils must be met and met adequately and promptly. No half-measures and peddling expedients will do. The covering in of a well here and the laying down pipes there may answer a temporary purpose. Such devices may lessen the chances of infection from one source. But they are akin to stopping a small gap at one part of a rotten fence and leaving whole panels down a few rods off: so that while a goat may not push through at one corner, the town herd shall stray in easily all round. The fence, or *cordon sanitaire*, of the permanent Indian cantonments, must enclose a far larger area and must be a much more perfect safeguard, than the present inefficient makeshift or delusionary hedge. This will involve questions of native policy, military requirements, and national outlay—matters over which the sanitary authorities have no control and upon which they may not be consulted. The medical staff are told that such and such stations are to be put on a footing of the highest possible sanitation with such and such means and appliances; and they have simply to obey orders and do their work in the best way they can with the tools and materials. But the question must be considered in a wider sense.

220. In the first place the position of military stations is most material. Some of these must be in the very hot-beds of disease germs;—that in the Deccan, for instance, alluded to by Professor Maclean. Assuming that the barracks at Secunderabad must be maintained as a military necessity—that it is a post of such importance, in fact, that the station cannot possibly be shifted elsewhere within a few miles—the Indian Government will have to insist on the same kind of modification of the native custom that has been suggested as regards Calcutta. This may be a difficult, a costly, or a hazardous, undertaking. But unless disinfection and deinfection be complete and thorough within a radius of four or five miles of the barracks, it is absurd to suppose that any internal organisation at Secunderabad will prevent epidemics of dysentery, or cholera, among the troops. The vivid picture drawn by Professor Maclean of the conditions surrounding this pest-house, is a full explanation of the malaria which envelopes it. And what possible escape is there in the barracks from the mist-borne germs issuing from the ravines; or from the reeking excrement-covered earth? I know nothing of the water-supply; but if it be on a par with that at some other stations, what are the chances of its being kept undefiled in such a neighbourhood? That there is a good, solid, tangible result from barrack hygiene has been seen. But barrack hygiene can avail nothing against the invasion from with-

out. And the question must eventually resolve itself thus:—either Secunderabad must be broken up; or the natives surrounding it must be coerced as suggested; or the regular per centage of those at the station must be infected. As Napoleon said:—"*L'homme meurt partout.*" What applies to Secunderabad, applies probably, with such modifications as may be called for by local peculiarities, to most other barracks and cantonments. A military station is taken up in the heart of India, and a native village is within a stone's throw and native huts are stuck all round like so many barnacles. And whilst this state of things lasts, how is the medical officer to reduce his mortality below a certain point? He may do everything that the regulations demand and something more. He may hunt about the cantonment and ferret out every collection of filth and garbage. He may get the executive at last to move an objectionably placed latrine. He may insist upon the necessity for this alteration in the accommodation for the men, and for that change of site for the horses. And at last having, by dint of fussiness and worrying and making himself generally obnoxious all round, got everything into perfect order and the place as clean as a new pin, dysentery suddenly breaks out, or cholera stalks in, and to his perplexity and amazement scores of victims pour into his hospitals daily. That per centage of mortality which he had been hugging himself he had reduced far below the average, is soon raised to near the old standard of mortality. To add to his discomfiture he may receive a hint that the high death-rate at his station would seem to point to some defect in the arrangements somewhere. He can only cover his defeat by "malarious influences "arising out of peculiar atmospherical conditions," and so forth. Now if both the external native and internal latrinal excrement difficulty be not got over, these malarious influences must be calculated on as constants. But as they are made by man, so they may be unmade by man. And atmospherical conditions will then be found inefficient to produce specific diseases.

221. No native villages or huts should be suffered within five miles of a cantonment—unless the natives consented to conform to [future] British notions and requirements in this matter. When this is carried out, deinfection may be carried out, and not until then. Air and water may then be kept free from pollution. There is one peculiarity in connection with the excrement-disposal of the natives which I fail to apprehend, and that is, why they do not utilise the material. Is this from ignorance, or from any idea of defilement or impurity? If from the former, a little education might do something. But I fear it is a question of purity. They are too shrewd and observant to have missed the effect of manure upon vegetation. One other point I may allude to. The Madras coolies employed on some of the coffee plantations in Ceylon set apart a place for themselves at some distance from their huts. [236.] It would be most interesting to learn whether this practice is universal in the district from which they come; and, if so, whether it originated among themselves, or was derived from the

British. It would moreover be extremely important, supposing this salutary modification of the Indian habit to prevail to any extent in the Madras Presidency, to trace out, if possible, whether it has had any appreciable effect in reducing the amount of infection in the district in which it obtains. Another thing I have been unable to get any precise information about. Does the custom of the Hindoo which is the burden of this theme extend throughout the whole of British India? Do the hill tribes follow precisely the habit of the natives of the plains in this particular? Or is there not, in the whole extent of the Empire, one section of the people free from this noisome and deadly mode of disposing of their excreta? If there should be any exceptional portion of the country —if it should happen that any race departs from the otherwise perfect catholicity of this observance and substitutes some usage by means of which its excreta are deinfected—I would draw special attention to that portion of India. For I am convinced of what must be the inevitable result as regards the decrease of malaria in such a district. I feel persuaded that an enlightened investigation would bring out the law there as everywhere else—that malaria is not an indigenous quality but an artificial product, of which human excrement is the main factor.

222. The greatest difficulty during the next half century, probably, will be found in solving the problem of marching through the interior of India with the least possible danger of infection to the troops. This danger there is no escape from altogether, and cannot be, until the whole of India shall be deinfected. The cities and military stations may be rendered safe by a vigorous and firm administration of the Empire. A few years may not only make them habitable by Europeans, but wholesome place of residence. But those whose duty it will be to go through the unsanitary regions must, of necessity, take their chance. They will have to face disease and death as they have always done. The question is to reduce the risk they will have to undergo to the very smallest. I cannot suggest any details to add to the present carefully devised regulations for troops on the line of march, except such as will readily suggest themselves to those who may be disposed to consider my hypothesis of cryptogamic infection to be worth anything in its present crude condition, and with its imperfectly sustained argument. It cannot commend itself, all naked as it stands, to the judgment of men perhaps; yet some may be induced to consider it worthy of consideration so far as to put it to the proof. Of course the general principles that would be evolved by the final acceptation of the hypothesis, would lead those in charge of marching troops to avoid native villages as much as possible and to guard against the source of danger in water. And this latter will always be the greatest practical difficulty. It may be feasible to give villages a wide berth, as a general rule, and to camp beyond the sphere of malaria. But the water is poisoned far and near at some seasons. There is no saying whither the germs of disease may not have found their way; and there is no

security anywhere. If I did not know something of bush life, I might have suggested that the men should not be allowed to drink along the line, without the organic matter in the water being first precipitated by some ready chemical process, or destroyed by boiling, as in tea or coffee. This would ensure a certain degree of safety no doubt. But I know too well that no considerations would long restrain men suffering from heat, and with a parched tongue and dusty mouth, from getting at water whenever they could. Such men as constitute the British force cannot take in the proposition. Tim Doolan drinks his fill at every chance for two or three days and laughs at his comrades who obey the regulation. A few at length join him and, as they escape, the greater number become demoralised. Sooner or later they fall in with a belt of cholera; they all take their prohibited poison draughts as usual; and they are all writhing with cramps within so many hours. No matter how often this kind of thing was repeated these men would not heed the lesson. Therefore no immunity from cholera, dysentery, typhoid, or remittent, germs in water, can be expected or relied upon. Tim Doolans will poison themselves in their own devil-may-care way in spite of all you can do to prevent them.

223. In leaving the subject of cholera, I beg it will be understood that I do not profess to have demonstrated its causation, and that I am perfectly conscious of many defects that may be found in the argument. The absence of any one single new fact must detract from the force of what is advanced. But I have had nothing but old material to work with and rearrange. And the whole is admittedly a speculative thesis—a pure hypothesis—with what amount of soundness in the induction remains to be seen. I must further point out that this view of cholera is not to be taken by itself, and considered apart from the other views in the work. It is too weak perhaps to stand alone. But taken in conjunction with the other facts and arguments, the hypothesis of causation appears to me to be built up on a firm and strong, though a light and slender-looking, foundation.

ON

DISINFECTION AND DEINFECTION

IN RELATION TO

YELLOW FEVER.

224. Although the deinfection of a city for *Yellow Fever* is not of such importance to European interests as that for cholera, it is yet a question of great magnitude and absorbing interest over a large portion of the globe. This is another of those tropical diseases the origin of which has been veiled in so much obscurity as to have given rise to endless controversy. The peculiarities observed in the manner of its propagation—it never having been known to occur spontaneously, or sporadically, in any of the cities in which it has nevertheless become established—has led to the belief that it is incapable of originating *de novo* in any part of the world. I cannot understand this view, and strongly suspect that where the conditions are supplied yellow fever will be a constant. However, the fact that the opinion is entertained, indicates that the source of the disease is probably of very limited extent, and that its germs can be generated only by factors rarely brought together within a circumscribed area. The greater the reason, therefore, why cities to which this affection has been conveyed should disinfect, and why those to which its spreading is imminent, should deinfect.

225. The first essential to determine is causation; and the history of the disease points to fœcal matter exclusively. Every place to which it has extended, and in which it has become domiciled, has afforded the requisite material abundantly; and wherever, in more modern days, cities have struggled to free themselves from pollution by sanitary arrangements, the measure of their success has been in a ratio to surface cleanliness and excrement removal. Rio Janeiro will serve to illustrate this; for the malady did not arrive there until within these five-and-twenty years and has been almost got under now. The excrement-disposal of Rio after occupation by Europeans was peculiar. The black and mixed populations followed the usual plan in such countries of disposing of their excreta, but the houses of the city were all furnished with tubs, of such a convenient size as to admit of a negro carrying them on his head and emptying their contents into the Bay. The whole of the excreta of the city were disposed of in this fashion; and the result was that, at certain points along the margin of the

most lovely piece of water in the world, the deposits accumulated so that pestiferous masses of excrement remained far above all tidal levels. The immediate consequences of such a state of things were of course enteric fever, in all its many shapes of bilious, gastric and remittent fevers, and the inevitable dysentery. Epidemics of these swept over the place as the local seasonal conditions determined. But for a long time the dreaded yellow fever kept off;—so long, indeed, that the residents at Rio thought they were to enjoy their immunity in permanence. Their fancied security, however, was dispelled at last; and when they were overtaken they suffered frightfully. For years the black vomit was allowed to run its course unchecked, and whilst there was no diminution of its cause there was no abatement of its virulence. By degrees the inhabitants awakened to a feeling that some steps must be taken. Efforts were directed to draining and scavenging; and precisely in proportion to the effectiveness of these measures in reducing the quantities of excrement available as a nidus for the fever germs, was their success in reducing their mortality. Unfortunately for them, as for many other communities in similar positions, they did not know what to aim at with precision. They wasted time, therefore, in non-essentials and applied themselves to organic matters generally, instead of devoting themselves in the first instance to fœcal matter specially. Ten years ago they were still adhering to their primitive and disgusting method of excrement-removal. But all this is changed now; water-closets have been introduced and an improved system of disposing of their excreta has been adopted. Although yellow fever has not been entirely got rid of, it is no longer formidable or unmanageable. Epidemics are of the past. Whether Rio can ever completely eradicate the disease will depend upon the amount of control it can exercise over its population. Your black slave is a thorn in the side of civilisation in many ways.

226. Yellow fever has visited Europe on several occasions. It has appeared in some of the seaports of England and, after causing a few deaths, it has disappeared altogether. The only European countries in which it has been enabled to make a stand have been Portugal and Spain and the Mediterranean—Lisbon especially was afflicted with a severe epidemic on one or two occasions. This limitation of the fever in Europe would appear to be due partly to temperature and partly to the presence of fœcal matter on the surface of the ground. Lisbon was notoriously the filthiest town in Europe—except perhaps Cadiz—and Gibraltar was bad enough—up to a comparatively recent period. Now, I believe, its condition is greatly improved, and the dirtiest places left are in Italy. Rome, Genoa, Naples, and Venice, are more fœtid and noisome from human excreta than other continental cities. But Lisbon at the time of the yellow fever was a loathsome city, where all the ordure and filth of the population was thrown into a sluggish stream, or standing pool, in the middle of the streets, and masses of excreta were allowed to decompose in one spot, un-

covered by water, for days. Add to this state of things excrement-sodden soil, and the conditions for yellow-fever probably could not well be more perfect. Altitude limits the range of the fever. Humboldt has some interesting accounts of this and other matters relating to yellow fever in his Travels (Chap. XI.); but I cannot stop to allude to them further than to point out that all the observations he made and the facts he collected, go to bear out the view that the malady is dependent on excrement, though Humboldt himself merely includes it as one among the causes. He notices how it is invariably surrounded with other fevers (typhoid), and how it differs in certain localities—varying so much, indeed, that he makes a distinction between yellow fever and black vomit. He also alludes to a fact which has been observed by others, but which has not been explained; namely, the comparative immunity of black and mulatto races. White people die in a proportion of more than two to one of a coloured population, from yellow fever. This peculiarity must have its physical explanation. There is another set of conditions, as regards cholera and the races of men, which I have not met with in books, but which I have on excellent authority. When epidemics of yellow fever occur in the West Indies (where it appears to be endemic, and where it probably started, even if it does not originate there now), the negro population suffers least and the Europeans most,—the half-castes taking an intermediate position. But when a visitation of cholera takes place, this sequence is reversed and the black man is terribly afflicted.

227. All the accounts of yellow fever appear to indicate that its cause is to be sought for in fœcal matter. If this be so, I conclude that the active agent, the germ conveying the specific poison, is derived from a mildew on excrement. All the manifestations of the disease itself, and all the phenomena of its distribution, are quite compatible with the assumption that some form of vegetation occurring on fœcal matter is the efficient cause. The mode of its growth I, of course, know nothing of; but, as a matter of induction, I assume that, as it occupies fœcal matter to the exclusion of the (hypothetical) cholera mildew, [198] it overspreads excrement and is converted into an aquatic plant under somewhat similar conditions to those required by the (hypothetical) mildews of cholera and enteric fever. The great variety of symptoms in the epidemics of yellow fever may be accounted for by the difference in the organic material on to which the mildew may find its way and the consequent modification of its poisonous qualities—precisely as it has been assumed in the case of enteric fever. Air polluted with portions of the plant at separate stages of its growth, may also produce different sets of symptoms. But I must leave those who are more nearly concerned to work out the whole matter for themselves. If the mildew be found the problem of deinfecting cities for yellow fever will be theoretically solved, and may soon be practically demonstrated.

testimony tends the other way; and the reasons for concluding that the unknown factors come from animal substrata are the stronger. For there is, first, the general impression based upon extensive observation in divers parts of the world. This prevalent notion derived from widely separated countries is strong presumptive evidence. Then the history of the disease is more in favour of an animal, than of a vegetable substratum. For granting that germs developed on the latter are theoretically capable of giving rise to the manifestations of remittent, yet the symptoms on the whole are more in accordance with those found in affections clearly dependent on animal substances. The disturbances of the brain functions are more nearly allied to those of the putrid fevers; while in extreme cases, such as are of constant occurrence in India and many tropical countries, the vomiting of black biliary matter and the intensely jaundiced condition of the body, are so similar to the effects produced by the poison of yellow fever, that some acute observers have been under the impression that the two diseases are identical; and that such variations as were found in the East and West Indies were due rather to some local modification than to a specific difference in the causation. Indeed, but for the mode of propagation of the Western fever, by infection, or contagion, it would be difficult to point out an unfailing sign by which the two diseases might be distinguished. For the ague plant may be present in both. All things considered, the most reasonable conclusion, from the present state of our knowledge, would seem to be that the unknown germs of remittent fever are generated from decomposing animal substances.

232. Adopting this view, questions naturally rise as to the form of the material, the kind of decomposition, and the nature of the emanations springing out of these. What are the factors and what is the product? Dr. Aitken, in the Third Edition of his valuable work (1864), speaking of malarious fevers generally observes:—"In these forms of fever a malarial poison of an "unknown kind, generated in paludal regions or litoral districts, "is absorbed, and affects the blood, as cholera, typhus, and other "miasmatic poisons do. The poison in the absence of any better "name, is known as '*malaria*;' and as physicians have merely "inferred the existence of such a poison, no exact knowledge has "yet been obtained as to its nature and source. Indeed, it still "remains to be shown that *malaria* have a substantial existence. "No poisonous principle has yet been chemically demonstrated in "the air of malarious regions. But many other acknowledged "disease poisons are in a similar predicament as to proofs of their "substantial existence; and the general impression with regard "to *malaria* is, that it is presumed to exist as a gaseous fluid in "the atmosphere of certain regions." It is not yet ten years since these views were given forth, but what a stride the revelations of the microscope have enabled the scientific world to take in that short time! From what a state of darkness into what a condition of light has this one subject been thrown! For assuming this

passage accurately to represent the views of the faculty—though I scarcely think it to be a fair reflection of the more highly educated opinions of that day—there will shortly be but few minds that hold to the belief that "gaseous fluid in the atmosphere of certain regions" can be the efficient cause of infectious or contagious disease—or of any disease of the zymotic class. After Dr. Aitken wrote, Dr. Salisbury soon led the way towards obtaining some "exact knowledge" of *malaria*—the substantial existence of which poison had, theretofore, been merely inferred—[or, rather, Dr. Salisbury had already demonstrated the existence of the ague plant, although the detailed account of his investigations, made in 1862, did not appear until 1866] and the rapid accumulation of evidence as to poison germs generally, since that period, can leave no reasonable doubt upon minds capable of inductive reason, that every disease, of the type now under consideration, has its substantial, specific, poison, in the shape of an organised substance, possessing vitality, or having the property either of inducing change in the fluids of the body, or of self-multiplication within the human system. Dismissing gaseous products then, what are the factors of the unknown half of remittent fever?

233. The animal organic matters to be found in the jungles of India, or in the swamps of tropical countries, may easily be classified, however varied may be the countless forms of life that go to make up the totality of putrescence in any given region. They range themselves naturally into two groups—organic matters from the bodies of dead animals, and organic matters contained in the excreta of the living. The masses of decomposing substances will of course be variable as are the country, soil, altitude, vegetation, and all the other local controlling influences which determine the collection of animals, or their remains, at a particular spot. But wherever surface drainage causes shallow pools, or lagoons of stagnant water, in hot steamy latitudes, there the elements of life and death are most abundant. The question now is from which of the two kinds of putrid matter—that of the dead, or that of the excreta of the living—come the germs of remittent fever? Without data all is but guess-work. Still, picking up again the thread of the argument used in discussing a similar aspect of the question as to the causation of enteric fever [166, &c.], I come to the conclusion that the excreta of living animals are the most potent factors of the unknown poison of remittent fever. It is needless to repeat the reasons why I deduce that decomposing tissues are, of themselves, inefficient causes of malaria. It is sufficient to say I believe that a mildew on the excreta of animals will eventually be shown to be the poison of remittent; and that foremost among the animals contributing their quota to the sum of the poison-forming material, I place man. I do not mean to imply that human agency is to be traced in the deadly effluvia of every swamp, or in the poisonous emanations from every patch of alluvial soil laden with animal matter, either on the surface of the earth, or in its lower strata [which may give rise to remittent by being un-

covered and by thus calling its latent sporules into activity, or by underground communication with drinking water]; but, while it is not unlikely that the dung of animals may develope mildews of innumerable kinds, some among which may have the property of causing peculiar morbid effects upon man's body—effects which, when conjoined with periodicity, are recognised as local remittent fevers—still I believe that human excreta are responsible for the largest share in the production of remittent fever throughout the world, and remittent fever of the worst type. For confirmation of this view, I point to the history of the disease as presented in Europe in the past, and as it is now seen on some parts of the continent. If the reader will turn to any of the accounts of the camp diseases during the wars of the last century, or the beginning of this, he will find that remittent under certain local conditions was as decidedly a constant, as dysentery was under other conditions. Setting aside the Peninsula, where the climate is warm and animal life more abundant, let him take the Low Countries, where the colder air is not so conducive to the creation of living things. What was the efficient cause of the terrible remittent at Walcheren and elsewhere, but the ague plant and the mildew from human excreta? And what but this combination is it that produces the well-known paludal fever which empties Rome of her tourists at certain seasons, and at times puts a stop to all travelling in the region of the pestilential marshes of Italy?

234. The remittent fevers of the Mediterranean, of the Levant, of Italy, Holland, Spain, and of the whole continent of Europe, in fact, I believe to be neither more nor less than *ague* plus *typhoid fever*. Perhaps in some of the colder parts some few cases of *typhus* may be complicated by the ague germs and may be taken for a special variety of remittent. But the larger number of cases, I suspect, are cases of genuine typhoid occurring from excrement-polluted vapours, bearing, in addition to the typhoid mildew, the germs of the ague-plant. The fact that enteric fever may be overlapped by ague has long been observed. Trousseau has referred to its occurrence in France, and Dr. Davis, in his account of the Walcheren Fever, has shown it, conclusively, by the intestinal lesions in fatal cases. But independently of these recognised cases of enteric fever with intermittent symptoms supervening, I suggest that all the other cases in Europe grouped under the generic head of remittent fever, are in reality unrecognised cases of enteric fever with ague—except the small percentage of typhus cases. Whether this assumption be correct or not depends, in a great measure, on the soundness or otherwise of the hypothesis advanced as to the mildew of enteric fever. If that be sound, I see no reason why this should not hold good. The variations observed in typhoid, when uncomplicated even, are such as to render the nature of that disease extremely obscure in many instances. It need not excite surprise, therefore, if the additional obscurity thrown over an imperfectly developed case of enteric fever by the presence of ague germs in the system, should

mask the typhoid symptoms and divert attention from the typhoid origin of the affection. If these views as to the common source of enteric fever and the unknown part of remittent fever hang together, it may easily be understood why both diseases should be so remarkably erratic in every clime. This mutability was observed in Europe in the remittents which accompanied armies, even though the source of the poison must have been pretty uniform as regards material. It is also marked in the spots where fever and ague now occur—some local epidemics differing from others in the neighbourhood, and also differing from those of preceding years in the same place. In fact the disease in Europe has been found just the same in these respects as enteric fever; though probably because it has been restricted to circumscribed areas and has not played the same important part in the bills of mortality, it has not attracted so much notice as typhoid. But the changes in the European forms of the disease are trifling, as compared with the multiplicity of changes found in the torrid zone and in the countries bordering on the tropics. There the shades and contrasts are infinite; and almost every region has its own special and peculiar remittent. As in typhoid fever it was suggested that the various phases depended on the variation in the mildew substrata, so it may be that the striking changes in the symptoms of remittent are to be explained in the same way. Mildews of the same family spreading themselves under the favouring conditions of heat and moisture over all the substrata in their vicinity, may easily become modified as to their poisonous qualities. The fauna of a country may thus, by some peculiarity of the excreta, produce such an alteration in the character of a mildew as to amount almost to a specific alteration. Hence the eccentric symptoms of the remittents of different countries:—the only unvarying manifestation being the periodicity. Ague, in one thing, is like dysentery: its germs have the same facility of invading bodies labouring under any form of disease. So that as all febrile affections in every part of the world, no matter by what cause produced, are set down as remittents if they develop intermittent symytoms, the diversified nature of remittent is easily explained. In colder and temperate countries ague may be conjoined with typhus, relapsing fever and typhoid; in warmer and semi-tropical regions it may ally itself with typhoid; while in the hottest it not only may be found with typhoid, but also with every nondescript fever having an analogous cause with typhoid. If to all these sources of variation be added the unknown controlling, or masking, effects of a quotidian, tertian, or quartan, ague, when grafted on any disease, the range of anomalous symptoms is enlarged to any extent.

235. The opinion expressed that the remittent fever of Europe is mainly dependent for its causation upon human excreta, is borne out in a very remarkable manner by the history of the peculiar remittent which has made its appearance of late years in the Mauritius. As the severe epidemics of various kinds which have

swept over this island within a short period illustrate many of the views herein expressed, I will give a brief, and necessarily imperfect, account of some of the more prominent of the conditions surrounding the excessive mortality to which its population has from time to time been subject. Perhaps the world in general might learn a useful lesson by a careful philosophical investigation into the causes of the diseases of the Mauritius. For nowhere in modern days has such an extensive experiment been carried out with excrement, as there. And nowhere could the direct connection between excrement and disease be exhibited so conclusively, or on so large a scale. Cause and effect could hardly be shown in closer relation.

236. Some five and twenty years ago there was not a more delightful or salubrious spot under the tropical sun than the Mauritius. Port Louis of course had its few sporadic cases of enteric fever and dysentery, as will always be the case in warm latitudes among mixed populations. White children also suffered from the infantile disorders incidental to the tropics. But there were no epidemics in the capital, then containing a population of about 45,000, while the interior of the island was free from epidemic zymotic diseases. Shortly before this, as I gather from the rapid glance I have had to throw over such detached information as has come in my way, the sugar planters had commenced the importation of coolie labour from India. On most of the estates, the planters—being evidently unacquainted with the customs and conservative notions of the Oriental race they were introducing—erected latrines for the two-fold purpose of preserving decency and securing the excreta for fertilising the land. To their surprise and disgust the Hindoo refused to enter these places. Threats and entreaties were alike unavailing to break down the barrier of caste. The Indian would have died placidly, but he would not pollute himself. He persisted in the habits of his forefathers and left his excretions on the surface of the earth. Unfortunately for the Mauritians the class of coolies sent to them seems to have been inferior in intelligence to the Madras coolies obtained by the coffee planters of Ceylon. These latter, while following out the same principles in connection with this corporeal function, yet observe some system in the matter, and thus deprive the practice of one of its most objectionable and dangerous features. They select and appoint a place for themselves at some distance from their habitation, and they constitute it an offence to their own community to resort to any place outside the proclaimed boundary. And to ensure obedience to this self-imposed law they affix a penalty to infringement, which they rigidly enforce. Now although even this arrangement is an undoubted evil and may possibly end in some fearful disasters, yet it is clearly less likely to poison the air and water of a whole country, than the abominable and deadly practice of the coolies in the Mauritius, who, as I understand, having no similar organisation, leave the substratum of contamination wherever chance may take them.

237. The results of sowing the seeds of disease broadcast over the island were soon visible—though not understood. At first there were several little independent endemic outbreaks of the inexorable dysentery, and of enteric fever, on the different plantations. When the seasonal conditions favoured propagation, and as the coolies came pouring in to swell the population and to add to the mass of infection, these separate sporadic attacks would spread and coalesce into small epidemics here and there throughout the country. As years rolled by and the importation of coolies went on unceasingly, keeping pace with the increasing demand for labour, these epidemics of dysentery and fever assumed larger proportions and became more and more serious and alarming. Port Louis, where the coolies were received on arrival and necessarily detained for a while before being drafted off to the plantations, was yearly more subject to low fevers and malignant dysentery. From a healthy pleasant tropical town it soon got to be considered malarious and dangerous. The first visitation of cholera, after the coolies, was in 1854, which carried off 115·8 per 1000 of the population. This was succeeded by another in 1856—a milder one that only killed 83·6 per 1000. During the next ten years following the latter pestilence, the seasonal conditions seem to have been unusually favourable for the population, and unfavourable to the development of poison germs on a very extensive scale. For with the exception of a formidable kind of fever, popularly known [instinctively] as the "Bombay Fever," which was probably a tropical and aggravated form of typhoid, there was no frightful outcome from the accumulation of surface fœcal matter constantly going on all over the island. The people seem to have become resigned to a high and steadily increasing death-rate. No systematic efforts were made to reduce the quantity of decomposing organic matter. Those who could, got out of the town, and those who could not, accepted their fate as a necessity of the position. Dysentery and fever were regarded as a natural consequence of malaria; and malaria was looked upon as the result of conditions over which they had no control. A few reflecting minds may have detected the true sources of the pollution that weighed upon Port Louis, and may have pointed it out and suggested plans for the removal of the evil. But what community ever listened to theoretical notions about hygiene, until it had had a practical illustration of the necessity for sanitary measures in the heavy penalty paid for neglecting them? The Mauritians did precisely what all highly civilised people do under such circumstances. They let things alone and waited.

238. The storm burst over the island generally, but discharged itself principally on Port Louis, in 1867, in the shape of a fever plague than which no more fatal epidemic is recorded in modern annals. The census of the town was taken in 1861. The population was then 74,416. In 1866 it was estimated at 80,000. From the 10th of February to the 31st of May, 1867, no less than 15,990 deaths were recorded, and of these, 13,488 were set down to

fever—the remainder being ascribed to dysentery and other diseases. Now assuming the population to have been 80,000, and taking the death-rate, in fever only, as one in three of those attacked—and a very high rate this would be—it will be seen that more than half the people must have been down with the fever. Adding the deaths from other causes to those from fever, brings out the result that one-fifth of the whole population of Port Louis was swept off in less than four months. And when it is mentioned that this total by no means represents the sum of the mortality, inasmuch as the period taken was both preceded and followed by an excessive death-rate for many months, it will be evident that there has been no such plague as this on British territory since the Great Plague. And even it was probably not much more deadly—though there are no means of drawing a comparison with accuracy. De Foe gives the numbers of deaths from August 8 to October 10, as 59,918 from all diseases, and 49,605 from the plague only. Speaking of the year, he says, " I saw it under the hand of one " that made as strict an examination as he could that there " really died a hundred thousand people of the plague in it that " one year; whereas, in the bills, the article of the plague was but " 68,590." De Foe himself believed that the larger number was nearer the truth. Accepting this even and estimating London to have contained about half a million people (in 1685 King put the number down at 530,000), the deaths in Port Louis in 1867 were proportionally not far short of those in London in 1665. In fact, all things considered, it may be fairly doubted whether the fever-stricken city was not more terribly punished than the plague-ridden one.

239. The pestilence which fell upon Port Louis was preceded by meteorological changes and conditions such as have been observed to go before great epidemics. Remarkably heavy and continued rains swamped all the low-lying lands and washed down the old and recent excreta of thousands from the slopes of the mountains encircling the town. The Dyot and Bathurst Canals, supplying Port Louis, were polluted at many points by the storm waters. The former, according to one authority, was also " contaminated " by the drainage of an extensive Indian village at Coromandel;" while the latter, according to another, was defiled "along its " course through the town by the free ingress by percolation of " the drainage of the streets, the organic matter being derived " from sewage and the carbonate of lime from the mortar of the " masonry with which the canal is built." The Grand River also appears to have been converted into a torrent of filth. Here then were concentrated on this doomed town not only its own accumulated excreta, but the contributed accumulations of excreta of a wide district inhabited by an Indian population. There is no occasion to seek further for the explanation of the fever plague of 1867. The air breathed in Port Louis must have been murky, and the water drunk must have been turbid, with typhoid germs. If the mildew view is correct, the amount of substratum present

must have led to the evolution of an enormous mass of the cryptogamic vegetation—both terrestrial and aquatic—which causes enteric fever.

240. By slow degrees the fever at length abated. The pabulum diminished and the disease lessened. The potable waters, by running off, and from receiving no more than their regular supply of excreta from the usual sources, became diluted and resumed probably their former condition. They were no more impure than before the heavy rains. The mildew-forming power of the extraneous excrement on and in the soil became in time exhausted; and as there were no further material additions from without, the town and suburbs had then merely to contend against the stagnant waters, [containing excrement and other organic matters brought down by the floods] together with their own excreta. The result was that though the full force of the epidemic expended itself in five or six months, there was still a very large mortality from enteric fever. The death-rate from typhoid remained remarkably high and would have been considered a formidable matter in Europe, or in any country that had not passed through such an ordeal as the Mauritius. In fine, typhoid established itself so that in spite of all efforts, hitherto, to shake it off, it has remained from that day to this; now swelling into serious proportions and again shrinking within more moderate limits, in obedience doubtless to certain natural, though at present indeterminate, laws.

241. In the following year the unfortunate Mauritians had another legacy from the storm waters of 1867. A strange disease appeared among the residents in and around Port Louis—a disease that alarmed and mystified them. For it was extremely fatal and was accompanied by intermittent symptoms. In fact ague had become developed; and, with typhoid as its base, it formed the peculiar remittent of the Mauritius, sometimes called the "Malagash" or "Malagasy" Fever—from its likeness to the remittent of Madagascar. The explanation of this before unheard of miasmatic disease in the Mauritius seems, from all I can gather, to be this. The unusually heavy rain-fall brought down from the mountains a larger amount of vegetable matter than had ever been brought down before. Whether this was owing to clearing and greater cultivation, I do not know. But the presumption is that, from some cause, the water which found its way to the marshes at the foot of the mountains, carried with it, in suspension, more vegetable *débris* than usual. Either this, or the moist condition of the ground at seasons when it was ordinarily dry, gave rise to an exceptional exuberance of vegetation at certain places. As the water passed off by evaporation and drainage, the conditions for the generation of the ague plant were brought together, as well as those for the typhoid [hypothetical] mildew—for of course the waters brought down excrement. The conversion of the ague mildew into an aquatic plant being effected, as I assume to be the case with all mildews, the drinking waters of Port Louis have most

probably become tainted in some way with the double causation of remittent. So that, the germs being propagated and maintained in air and water, Malagash Fever has clung to the Mauritius ever since it first appeared.

242. Since 1868 another form of Oriental disease has declared itself, though it has hitherto been confined to the boys in the Reformatory. In this establishment malignant ulcers have broken out at times, with what result may be gathered from the fact that in 1871, from the 1st of January to the 1st of March, there were 25 deaths among 318 boys and there were then nearly 100 ulcer cases in hospital—half of which, the account before me states, must be fatal. Whether the boys died, or whether the disease was stayed and has not returned in the Reformatory, or elsewhere, I have not yet learned.

243. For those who may be engaged in tracing out the hidden cause of diphtheria, I may mention that in 1871 this disease appeared at Curepipe, one of the healthiest spots in the Mauritius; one looked upon, indeed, as a safe retreat from the infectious disorders incident to the capital. There, however, diphtheria came, several children died and many families left the spot in consequence. I know no more than the bare facts stated, but they may be worth following up.

244. In this hasty survey of the maladies inflicted on the Mauritius, I have taken no note of the struggles made by the people of Port Louis to avert the evils under which they have suffered. But they have not been idle. When the fever of 1867 left no room for doubt on their minds as to the intimate connection between excreta and the epidemic; when it was brought home to them so palpably that the sewage question was one of life and death to them; they set to work in earnest. But there, as elsewhere, the want of a clear perception of the precise causes of their diseases, led to conflicting views as to the measures to be taken and to a consequent dispersion of force. All were agreed, however, that the principal object was to get rid of as many sources of filth and pollution as possible. The result has been on the whole a marked change and visible improvement in their sanitary condition. I observe that the Legislature has long had under its consideration the subject of disinfecting and deinfecting Port Louis. Sir Henry Barkly's paper on the drainage of the town is fully up to the level of the scientific knowledge of that day, and is shrewd, practical and statesmanlike. But the want of definite knowledge of the conditions would probably have rendered the plan, to which the Governor gave his adhesion and hearty support, an inefficient one. Or rather I should say the drainage of the town, as proposed, though it would have been a valuable adjunct, no doubt, to other measures, and would in any case have materially lessened the amount of poison-forming material within the town itself, would have failed to provide for the malaria from the swamps or marshes, and for the mal-aqua of the canals. Any scheme of deinfection which does not include provisions to cut off these large sources of poison-supply, can never

secure Port Louis against the recurrence of epidemics of typhoid, or of remittent, when the seasonal conditions shall again favour an extensive evolution and propagation of the germs. At the present time I find that the Council is considering a proposal to tax heavily for sanitary purposes. The questions involved are complex and it would be absurd to offer any opinions at a distance as to details. But from my stand-point, I have no hesitation in laying down one general proposition. I believe that until the Indian population can be induced by some means to modify their habits, in the one direction herein pointed out, thorough deinfection of the Mauritius will be precluded. So long as surface contamination of the soil is allowed, hygienic efforts must fall short of requirements at all times; while at certain seasons and in some places they will probably be futile as regards the prevention of epidemics. Whether this proposition, however, is sound or not must be left to future observation, investigation, and experience.

245. The foundation of many of the views here expressed rests mainly on the written accounts of the various diseases of the Mauritius, by the medical men who observed them and reflected upon their causation. Upon the vexed question of the origin of the peculiar remittent, there seems to have been some difference of opinion. But it is not necessary for me to allude to this further than to show that the views nearly all square with the views here taken. Thus Dr. Beaugeard clearly connects the remittent with its paludal and typhoid origin, and traces its intermittent germs to the drying marshes. He further hits the nail on the head by suggesting that they should be either drained or drowned. Dr. Régnaud considers the fever one made up of different types, including Intermittent, Remittent, Continued, or Pseudo-Continued, Pernicious, and Bilious, or Bilious Remittent. Dr. Barraut has evidently preceded me also, by affixing the name of Typhoid-Paludal to the Malagash Fever. In fact it appears to me that whatever shades of distinction may have been drawn by writers on this disease, they have in reality been in accord as to essentials. The knights both recognised the shield, though each saw but one side. There is a remark in the report of Dr. Edwards for 1868, to which I think exception must be taken, if I apprehend the sense in which it is intended. Dr. Edwards had great experience of malarious disease in the Crimea and the Caucasus and he did good service in the Mauritius epidemic, [which he appears to have predicted by the way] and finally became attacked himself and had several relapses. His views as to malaria, however, as contained in the report on the Vital Statistics for 1868, were surely not maturely considered. Of malaria, he says, "we really know "nothing"—which may be granted. Then, after stating certain conditions required for its production, he adds that it is found "often co-existing with the products of vegetable decomposition, "although not proved to depend on such decomposition, and "entirely independent of animal decomposition"—which may not be granted.

246. There has been no theatre in which the phenomena of the causation of malarious diseases have been played out so clearly as in that of the Mauritius. Nowhere can the world turn for more complete or perfect representations of the power of human agency in bringing about great pestilences. And there is no school in which the epidemiologist may learn more than in the study of the future of this island. For under present conditions, it contains within itself a never failing and an inexhaustible supply of substratum for the germs of most eastern plagues; and it depends upon the dexterity with which the Mauritians extricate themselves from their position, what the next upshot will be. Not that they need fear that any disease which may come amongst them can surpass, or equal, in malignancy, the epidemic of enteric fever of 1867. There can be nothing more fearful than that. And seeing this, it would seem to be their wisest course to concentrate all their efforts on the means to render the recurrence of a typhoid epidemic an impossibility:—the more especially since the more perfectly they deinfect against typhoid, the more likely they are to shut out cholera, bubonic plague, yellow fever, malignant boils and ulcers and all the other plagues—as has been seen in Japan, and Japan only. All large deinfectant measures for enteric fever [or remittent — the same thing—plus ague] should however be preceded by a clear knowledge of the causation of the disease, or they will probably end in failure. They must go *au fond* and strike at the root of the thing. The Mauritians can no more hope successfully to cope with an unknown enemy, than can the various corporations of the large towns of England expect to deal with their excreta safely, before the precise dangers connected with excrement in bulk are understood. So long as any doubt remains as to the exact cause of their remittent fever, there must be divided counsels and natural hesitation about laying out large sums in sanitary schemes which may after all be in the wrong direction, or may do a minimum good at a maximum cost. It would be the height of folly to drain or drown their marshes, for instance, unless and until the question as to the intermittent part of their remittent is settled. And settled it should be beyond the shade of the shadow of a suspicion, and probably would be, if the sets of observations made by Dr. Salisbury were carefully and faithfully repeated by known competent men. This is clearly the first practical step towards the solution of the problem of remittent. There may be microscopists in the Mauritius skilled in mucology and vegetable physiology, and fully equal to the investigation of this subject. But the study is a special one and implies more learned leisure than is generally to be found in small communities—though there is no saying where original and independent minds may not be found working on abstruse scientific matters in any part of the world. If then the island contains a man of this kind on whose knowledge, judgment and probity, all can rely, it would be an economy to secure him at any price and give him the necessary time to conduct the experiments. Yet as the law about

prophets holds good, probably, in the Mauritius, it might be wiser on the whole to seek assistance from the Imperial Government. For the question is an Imperial one and affects not only the Mauritius, but Great Britain herself and all her dependencies. To determine accurately the relation between the excreta of large populations and the diseases of those populations, would be worth millions annually to England alone. And considering the peculiar, exceptional and pressing nature of the circumstances, the Mauritians might go with a good grace to the Home Government for such help in their extremity as may legitimately be asked and granted. If the notion of thus procuring external aid be distasteful to the people of the Mauritius, or thought superfluous, I have only to observe that I have thrown out the suggestion for cosmopolitan reasons, as well as for reasons affecting themselves. I do not take merely an insular view of their position, but I consider the questions that touch them so nearly are just as important, though more remote, to the world at large. Viewed philosophically it matters not where, or how, the great problem of enteric fever, which underlies the question of their remittent, is worked out. If in Port Louis by the Mauritians themselves, the solution will be as valuable as if it came from any European capital. The only really essential thing is that it should come quickly.

247. To return to remittent fever on its wider field. As strong presumptive evidence in favor of the mildew view of causation, I turn to the well known fatal remittent connected with the granite of Hong-kong. It seemed somewhat difficult to reconcile the occurrence of the disease there with the mode of origin suggested; and, indeed, I was unable to get at the explanation of the phenomenon, until I met with the following passage in Dr. Parkes's work on Practical Hygiene—a work replete with information well put and full of valuable references. Dr. Parkes says:—"The disintegrated granite of Hong-kong contains a small proportion of "organic matter, which could hardly produce malaria. Friedel, "however, has stated" [Ost. Asiens, von C. Friedel, 1863], "that "the disintegrated granite, which is highly absorbent of water, "becomes often permeated by a fungus, and it would be interesting to see if there is any relation between the development "of this fungus and the production of malaria." It would indeed be interesting, and something more. It might be a means of settling the largest question affecting humanity of the present day. For my part I believe the relation of this fungus found in granite by Friedel, to the production of malaria, to be a very close one indeed—no less, in fact, than the relation of cause and effect. Destroy mildew and I do not see how the malaria of remittent can possibly be produced.

248. It is stated to be a fact that the planting of gum-trees in many parts of Italy has had a sensible effect in reducing fever and ague in the paludal regions. Whether this has been determined with anything like scientific accuracy I do not know. On the assumption that some connection between the growth of the gum-

trees and the diminution of remittents has been made out, it becomes a matter of some interest and importance to ascertain how much of this is to be attributed to mechanical obstruction to the currents of malaria, and how much to some specific virtue resident in the particular tree. Has the Eucalyptus any inherent property by which it decomposes organic matters floating in the air? In the first place it may be supposed that the Italians have not found groves of other trees to have the same salutary effect, in checking fever and ague, that gum-trees have. If this be so, the gum is not a mere screen. What then is the agency by which it produces the result?

249. So far as I know the atmosphere of Australia has not been examined, either chemically or microscopically, anywhere. We possess, therefore, no precise data by which to determine what vegetable products are to be found in the air about gum-trees. Judging by analogy, however, and taking the rough test of our senses, we may reasonably conclude that the pungent aromatic odour around gum saplings indicates a considerable number of oily and resinous particles in the air. Are these effective in disinfecting malaria? Dr. Day's discovery that, in this climate, volatile oils absorb antozone largely, in the form of peroxide of nitrogen, leads to the conclusion that the aromatic particles emanating from our gum-trees are probably conveyers of antozone. As Dr. Day has shown that ozonic ether is a powerful disinfectant, by combining with organic matter and disorganizing it, it follows that the antozonic globules given off by gum-trees would act in a similar way. According to Dr. Parkes, [Practical Hygiene, p. 77], Becchi examined the air of the Languedoc marshes in 1861, and found organic matter in considerable quantity. "The amount," he says, "in Becchi's experiments was ·00027 grammes in a cubic "metre of air [=.000118 grains in 1 cubic foot]. Ozone, led "through a solution of this organic matter, did not destroy it." It would therefore appear, from Dr. Day's views, that allotropic oxygen is chiefly effective in the destruction of organic matter when in an antozonic condition—as met with in oils exposed to the effects of diffused light. This may be the explanation of the use of spices as deodorisers, antiseptics, and disinfectants, from time immemorial. The ancient belief in the efficacy of frankincense and myrrh would thus appear to have rested on the scientific foundation of the aërial combination of organic matter with peroxide of nitrogen. And the burning of incense has an equally solid reason for the practice. Thus it is too that fragrance and sweet smells are instinctively associated with salubrity. And on this principle it may be that a cordon of Australian gum-trees round the Pontine Marshes may not only intercept, but may destroy, a good deal of the deadly effluvium hanging over them at the sickly season, Though this is after all but an insecure and unsatisfactory tenure by which to hold existence in such a neighbourhood. It would require forests of eucalypti to ensure even a moderate degree of safety. For it must not be forgotten that the

gum-tree is not found to be a thorough disinfectant of malaria in Queensland, where simple ague is common enough, and I should be loth to trust entirely to its protecting influence in the vicinity of Rome. The Pontine Marshes are only to be rendered innocuous, as it seems to me, in one way—by drainage and cultivation. To be sure they might be converted into simple ague fields, if the Italian peasantry could be prevailed upon to deal with their excreta less after the fashion of the Hindoo. This would deprive the stagnant marshes of the animal element, and cut off the more deadly half of the causation of local remittent, or Roman Fever. And by this means these swamps might be partially redeemed and brought into the condition of the fens of Lincolnshire, as they used to be—nurseries for intermittent merely. Any one, however, who knows Italian habits, knows very well that it will take probably two or three generations to indoctrinate the people with the idea that there can possibly be any connection between excrement and disease. If, therefore, the part disinfection of the pestiferous marshes of Italy has to await a reformation of national custom, it will be some years yet before the remittent fever of the country will be reduced into ague.

250. It is superfluous, perhaps, to recur to India and to point out the most abundant sources of intermittent always present in that excrement-saturated country. Yet as I merely alluded to this disease incidentally in dealing with cholera, I now draw special attention to it. What, I would ask, is the explanation of that frightful pestilence in the Burdwan District which, according to the *Lancet* of 26th October, 1872, has been carrying off thousands of the native population? This Burdwan Fever, it seems, is precisely of the same form as that remittent they now have in the Mauritius—the Malagash Fever. It is unnecessary for me to say more than that I assign the same cause for these separate manifestations of disease. I know nothing of Burdwan, but I feel assured that precisely identical factors have been actively at work there as are now in operation at the Mauritius. The natives have supplied the typhoid half and floods have furnished the ague half of the epidemic. And so long as the Indian population befoul their country there will be no help for them. Varying crops of excrement germs will succeed one another as different kinds of weeds spread over the ground every year. This time the enteric fever fungus has taken possession of the Burdwan country; but being accidentally joined by the ague-plant, and the conditions being favorable to germ propagation, the result is an epidemic of what is known as remittent. Perhaps this view may be verified by those who have had opportunities of coming to a conclusion. There may be a portion of the district outside the influence of the ague germs, but to which the typhoid germs may nevertheless have extended. If it should happen that paludal malaria should have been cut off from the typhoid malaria anywhere, there may be well-marked distinctions in the epidemic. In one part there may be remittent with decided paroxyms of ague, and in another part there

may be pure typhoid—or typhoid as near the normal English type as is likely to be found in India. If the position of affairs admit of a careful investigation on this point, and it would require to be done by those thoroughly able to eliminate all sources of error, I feel assured that the result would be to establish the fact of the compound causation of the remittent, and to go a long way towards showing that the unknown portion of the disease is a form of Indian typhoid.

251. It has already been stated that I do not regard the intervention of human excrement as a necessary condition for the production of some phases of remittent fever. And even in India itself there may be some swamps and jungles entirely free from this form of pollution, and yet contain animal organic matter, and especially excrementitious matter, sufficient to give rise to malaria of a kind to cause certain descriptions of remittent. But having in mind the extensive fœcal contamination of the earth's surface throughout the country, I am disposed to conclude that by far the greatest proportion of the cases occurring in India, occur from excrement poisoning—the more serious and fatal cases particularly. There are multifarious kinds of remittent in various parts of the world in which the ague germs form by far the most serious portion of the disease and remain to afflict the patient for months after the other effects have passed entirely away. In some instances, indeed, the remittent attack lasts only for a few days, is very slight while it does last, and leaves abruptly—the intermittent, however, still clinging. In other cases again the patient gets such an overpowering dose of the poison that he succumbs in a few days—or passes through successive stages slowly and then sinks. All this divarication speaks of dissimilarity, or variation rather, of factors. I assume the milder shapes of the affection—or more correctly speaking the milder affections, for they are clearly distinct, specific diseases, though confounded and grouped under the head of remittents—to occur from the excreta of animals and the deadly ones from those of man. But it is not altogether improbable that among the unknown productions of nature there may be some more virulent and rapid poison than that formed by the typhoid mildew, but producing analogous effects and growing on the excreta of some other animal. It is not possible to say, either, what combinations of poisons may do in producing fatal complications, when singly they may be comparatively mild in their effects. Perhaps some rare fungoid growths never occur but when two or more kinds of excreta come together. It is not impossible but that an entirely new admixture of elements might produce a strange and specific cryptogam. For instance the accidental intermingling of the dung of the camel with that of the kangaroo might give rise to a peculiar kind of vegetation such as has not previously existed. This may have taken place in the interior of Australia for all we know. Had the introduction of the alpaca into Victoria been successful, a still more unlikely conjunction of material might have happened—the commingling of the dung of

this quadruped with that of the wombat. Mucologists, by availing themselves of Zoological Gardens, may yet produce startling results in vegetation by artificially arranging the excreta of animals. But not to dwell on supposititious regulated collocations, there has been an actual amalgamation, on an extensive scale, of the dung of animals of different countries, going on for years past in New Holland. In some parts of the country the kangaroos increase so rapidly as to be a serious evil to sheep-farmers and cattle-owners. Thousands of these marsupials herd with the sheep, or cattle, in the same paddocks, and consume or destroy the larger proportion of the grass. It must consequently happen that a large commixture of excreta takes place; and when this goes on for many years under all kinds of conditions, there is nothing wild in the supposition that at some period or other a new fungus may start into existence, which may propagate itself over a wide district and may possibly end in producing an unheard of catarrh among the flocks, or some singular murrain among the herds. Such blendings of the excreta of different classes of animals are very likely to occur at the swamps and lagoons at which these animals water in tropical regions; and it appears to me that this not only offers a very reasonable explanation of some of the epidemics that are known to occur among wild beasts, but suggests a solution of the infinite varieties of remittent.

252. But the animal of all others the most likely to be connected with the origin of remittents in hot countries is the ape. There are some former remarks [83] touching the evolution of the dysentery germ from the evacuations of these animals; and I may add that if it be shown that their excreta are so nearly allied to those of man that they will originate and support a fungus capable of producing dysentery, it will follow that the analogous diseases of man may be derived from the same source. Of course this has not been shown, nor has the subject been entertained that I know of. But I assume the direct connection between human excrement and dysentery. It has been seen that dysentery is common to all races of men in all countries in all times. Now the excrement of the larger simians has not been analysed, nor has the physiology of their digestion been very minutely gone into, to my knowledge. In the Lectures on the Comparative Anatomy of the Organs of Digestion of the Mammalia, delivered by the Hunterian Professor (W. H. Flower, F.R.S.), last year, I find some interesting and valuable particulars concerning the anatomy of the parts; but, unfortunately for my purpose, the consideration of their function does not come within the scope of the anatomist. The products and educts of digestion, therefore, are not specially treated of, although they are incidentally alluded to. The excreta are not touched upon. Yet the observations made upon the intestinal canal and the organs and glands connected with it, will afford means of judging as to the nature of the excrementitious matters—especially in drawing a comparison between those of the larger man-like apes and man. In his first lecture Professor Flower, after mentioning that

the intestines of animals show the crypts of Lieberkuhn and the duodenal racemose glands of Brunner, says—

“Other structures in the mucous membrane, about the nature “ of which there is still much uncertainty, are the solitary and the “ agminated glands, the latter known commonly by the name of “ ‘Peyer's patches.’ These were formerly supposed to be secreting “ organs, which discharged some kind of fluid into the intestine, “ but are now more generally considered to belong to the group of “ structures of mysterious function of which the lymphatic and “ lacteal glands are members. The solitary glands are found “ scattered irregularly throughout the whole alimentary tract; “ the agminated, on the other hand, are always confined to the “ small intestine, and most abundant in its lower part. They are “ subject to great variation in number and in size, and even in “ different individuals of the same species vary in character at “ different periods of life, becoming, like the mesenteric glands, “ atrophied in old age, though not to the same degree—a point “ which must be borne in mind in noting their condition in the “ dissection of animals.”

The digestive organs of the *Simiidæ*, or *Anthropomorphous apes*, [Lecture III.] would appear to be somewhat imperfectly known—from the difficulty of procuring fresh specimens. Of the *Chimpanzee* it is said “The stomach is large, in form much like that of a man.” * * * “The colon is very voluminous, and greatly sacculated by three strongly marked longitudinal bands, arranged “ as in man.” Of the *Gorilla* the one specimen in the Hunterian Museum, enabled the Professor to say that “the large intestines “ are of great capacity, and including the cœcum eight feet and a “ half in length; so that the proportions of these viscera are much “ the same as in man.” The *Orang* [*Simia satyrus*] comes next. “ The colon is proportionally larger and longer and more loosely “ attached than in man, and consequently more convoluted.” Among the *Gibbons*, the two animals referred to are *Hylobates leuciscus* and *H. agilis*. In these “the colon is greatly sacculated.” [I omit reference to other digestive organs.] At the end of this lecture Professor Flower says, “Before leaving the interesting “ family of Simiidæ it will be well to take a short review of the “ more important points noted in the anatomy of their digestive “ organs, especially in relation to the resemblance to man. * “ * * The stomach in all differs from that of man only “ in a greater elongation of the portion next to the pylorus and its “ more marked separation by a constriction from the rest of the “ cavity—a point on which there is, however, great individual “ variation in the human subject. They all differ markedly from “ man in the absence of the valvulæ conniventes in the small “ intestine, unless the statements of Vrolik in the chimpanzee and “ of Sandifort in the orang [which have not been verified by subsequent observers] can be trusted. The colon is proportionally “ longer than in man [perhaps Hylobates excepted].” In the fourth Lecture the length of the small intestine of the *Semnopi-*

thecus entellus is given on the authority of Professor Owen as 13 feet 6 inches—but there is no mention of glands. The *Baboon—Cynocephalus anubis*—has 110 inches of small intestine. "Its "mucous membrane shows no trace of valvulæ conniventes. The "villi are numerous, but soft and small. Peyer's glands are large, "about twenty in number altogether." As regards the large intestine;—"Throughout its mucous surface are scattered dark-"coloured follicles, of the size of pin's points, either solitary or "aggregated in groups of from two to six or eight in number, "arranged in an oval form, the long diameter of which is trans-"verse to that of the intestine." Going to the lower grades, of the *Ateles*, or *Spider monkeys*, the stomach of the *A. melanochir* "still "greatly resembles that of man in form." "The small intestines "are ninety-five inches in length. * * * There are "no valvulæ conniventes, but Peyer's patches are numerous and "large, especially in the lower half of the intestine; about 25 of "various sizes were counted in all." In the large intestine, "single dark-coloured closed follicles, and small groups of two to "four similar follicles, are sparingly scattered on its mucous sur-"face." In the common *Capuchin monkey—Cebus capucinus*—"Peyer's glands are numerous and large throughout the small "intestine from the duodenum downwards and in the lower part "of the ileum very long and following each other with scarcely any "interval. * * * The colon is comparatively small "and simple." There is but little more on the special point. The *Mycetes seniculus—(Howling monkey)*—has no valvulæ conniventes and the Peyer's patches are small and not numerous. The *Pithecia monachus*—one of the *Sakis*—has no valves. "Peyer's patches "(the largest one and a quarter inch long) are scattered at "tolerably regular distances all along the canal." The colon of the *Midas œdipus* "is disposed very much as in man." But there is no occasion to quote further. I will merely add that all the other organs and glands described have a similar analogy to those of man that the intestinal canal has. It may therefore be assumed that the processes of digestion and all the physical results, are as nearly allied as the structures concerned. Although comparative physiologists have not yet evolved much minute information as to the nutrition of the apes, yet it is a law that similarity of parts implies similarity of functions; and coupling this with the familiar fact that the larger apes can adapt their diet readily to that of man, the inference is that their excreta are as nearly alike as their organs. Chin, the well known Parisian Chimpanzee, lived like a human being, took his pint of Bordeaux daily, and thrived exceedingly for years. There can hardly be an essential difference then between the two organisms in all that relates to animal life. I should assume, indeed, that there was less difference in composition between the excrement of Chin and that of an European fed on the same food, than between the excrement of an European and that of a Hindoo. The European and Hindoo are both subject to dysentery, and, inferentially, Chin would have been subject to dysentery. The European and Hindoo get dysentery from their own excrement, or one from the excrement of the other. Therefore Chin would have got dysentery from the excrement of the European or the Hindoo. And, therefore, Chin's excrement under similar conditions was capable of giving dysentery to the European or the Hindoo. And if dysentery—why not typhoid fever?* Running down the scale from the chimpanzee to the smallest monkey known, the gradations of change in the excreta would probably be so insensible that no generic difference could be found anywhere—no difference, in fact, not sufficiently to be accounted for by a variation in food. Thus the *titis* and the *viuditas* of the Orinoco are connected with the *baboons* and *gorillas* of Africa. If this chain holds together, it seems to me that some of the more serious forms of remittent in those tropical regions in which man's presence cannot be supposed, may be accounted for. Yet there are undoubtedly large tracts of swamp in the world from which come malaria capable of producing fatal remittents and to which malaria neither men nor monkeys can have contributed—at all events to such an extent as to account for the quantities of the poison evolved. In such cases it must be assumed that other forms of decaying animal matter can sustain, or possibly originate, a description of fungoid vegetation having highly narcotico-acrid qualities. As a general rule, however, there are no such deadly remittents as those with which man's excreta may be connected. There is no country where these affections are more to be dreaded than in India and China. Such epidemics of remittent as the Burdwan Fever are not to be paralleled elsewhere—except perhaps in the Mauritius, where precisely the same causes cooperate.

253. In considering the causes which lead to the variable character of remittents, it must not be overlooked that the extraordinary difference in the excreta of man himself, in widely separated latitudes, may have its effect in modifying the poisons derived from them. This disparity may be the explanation of the uncertain shapes which typhoid assumes. There is a large diversity in composition, in consistence, and in other qualities, dependent on the peculiarities of the diet of nations. This marked character given to excrement by food, may be the means of furnishing one of the conditions required to bring forth a specific vegetable parasite.

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* Is there any history of apes getting cholera in India? This is a matter which may probably be determined without any difficulty, though I have not seen any mention of these animals being attacked. Theoretically they should undoubtedly be subject to the effects of the cholera poison, and the manifestations should be nearly identical with those observed in man. If this be the case—if the inference be borne out by the fact—the ape might be made of marvellous service to man in the way of elucidating the true nature of the specific poisons. Have the quadrumana in the Zoological Gardens ever had whooping cough? or small-pox? Has a monkey been seen in any country suffering from intermittent fever? I can conceive great results from following up the diseases of these animals, not only as to the nature of poisons themselves, but as to their *modus lædendi.* I have made diligent search, by the way, for any detailed account of the deaths of quadrumana in the *Jardin des Plantes*; but there is nothing to show that post mortem examinations were made in the Paris collection in 1859 (the dysentery epidemic year), as in the London Gardens by Mr. Crisp, and now by Dr. Murie.

Thus it may be conceived that a speciality in the fœces—the presence in them of some peculiar component for instance—may be the factor which determines the cholera fungus, or the yellow fever fungus, or the fungi of many of the various plagues of the world. Assuming that excrement disposed of in a certain way in certain parts of India are two of the conditions required to produce the cholera fungus, it is conceivable that a third condition may be a certain description of food. A particular kind of grain or fruit, some local ingredient in a curry, or possibly mouldy or mildewed rice, may be one of the factors required for the generation of the fungus. That fungus once formed and its sporules scattered in the air, seizes with avidity on all excrementitious substrata and flies and spreads itself in every direction; only existing, however, so long as it can procure the necessary pabulum and so long as seasonal conditions permit of its existence. When these conditions all fail and the water-plant dies out and the last crop of sporules germinates, perhaps, but comes to nothing, there is an end of the vegetation for the time being; and the only way in which it can be resuscitated in a foreign country is by a fresh importation of the fungus. It can only originate *de novo* in certain circumscribed parts of India. This localised source points clearly to some local peculiarity in the substratum; and I know of no more probable cause of this peculiarity than the one suggested—that of some special element in the food. It is possible of course that the strata of the soil, or the presence of some peculiar vegetable humus, may have something to do with determining the cholera fungus. Or it may be that years of putrefaction, or fermentation, of fœcal matter, and, that in a particular way only to be arrived at in a few districts—like the œnanthic ether of certain wines—are required before it will produce this parasite. Or again it is just possible that the dung of some local animal may be concerned; either starting this specific cryptogam of itself, or in combination with human ordure. Yet the most likely mode by which cholera becomes endemic in certain spots of India, seems to me to be through the *ingesta* of the natives, by means of which the *egesta* acquire a distinctive character. Whether this be so or not, however, we know that different races, and even the same individuals at different periods, void excreta differing materially in composition and in other things. We know further that different soils and climates produce corresponding differences in the same description of vegetation. It is therefore a warrantable inference that the typhoid plant growing on the excrement of flesh eaters in Great Britain may differ in size, form, and qualities, from the typhoid plant growing on the excrement of pure vegetarians in India. If this be granted, the strange modifications of typhoid may be understood; [170] and if typhoid be found to be one of the causes of remittent, a vast deal of the obscurity arising from the metamorphoses of that disease will be also explained.

254. A parallel set of reasons to the foregoing applies to the intermittent part of the disease. The ague-plant may be, and I

infer is, most undoubtedly, modified to a very great extent by the nature of the substratum on which it developes, and the country in which it grows. There can be no doubt that there is a wide scale of intensity of the ague principle; and this is regulated probably by the sun and the decaying vegetable matter on which the parasite occurs. In some instances the degree of constitutional disturbance produced by virulent attacks of intermittent is very great;—so great, indeed, that I strongly suspect the attacks have been confounded in many cases with those of remittent fever. When the violent effects of the first few days after inhaling or imbibing the germs of the ague-plant in some hot country—the jungles of India for example—have passed away and the patient settles down to the ordinary "shakes;" I fancy he is not unfrequently said to have had a mild touch of remittent fever, when in fact he has had a severe one of pure ague without any complication from germs derived from decaying animal matter. The suddenness described by writers with which the remittent symptoms disappear supports this view. Yet the whole subject is so obscure that there is no arriving at any satisfactory conclusions in connection with it.

254. Before quitting the subject of remittents, I must offer one or two remarks upon the entire absence of the disease from any part of New Holland;—inasmuch as it confirms, or tends to confirm, negatively, some of the views herein expressed. The singular freedom of Australia from remittent fever and malarious diseases generally has frequently been commented on; but I have not seen any explanation of this exceptional purity of the atmosphere which is to my mind a full and complete exposition of the matter. In January, 1851, Colonel Mundy * steamed up the river Yarra to Melbourne and remarks:—"The stream is narrow and lazy, and "near the town by no means pleasing to the senses; it runs "through flat banks, covered with 'fat weeds' and mangroves, or "other low scrub; and in any other country but Australia I would "have pinned my affidavit upon such a tract producing ague and "fever in high perfection. Melbourne, nevertheless, is, I believe "quite as salubrious as any other part of New Holland." The thought which occurred to Colonel Mundy is the natural one of all travellers, and I have known many an Indian officer down here on leave shudder at the mention of a day's snipe shooting. The existence of a tract such as the Koo-wee-rup Swamp with its thousands of acres of black loamy mud, overgrown for miles with rank sedge and reed beds only partially covered with water at some seasons, and the non-existence of the mildest form of ague among those who have lived for the last five and twenty years on the margin of this large mountain sewer, is indeed an apparent anomaly in nature, and one not philosophically accounted for. For fifty years after Australia was colonised intermittents were unknown; and it was not until settlers had pushed up into Queensland and occupied the country that it was discovered that,

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* Our Antipodes.

in the tropical parts of the continent and in the latitudes closely bordering on the tropics, ague was indigenous. It was then found that in certain localities where the herbage was rank—as in shaded gullies, in marshy flats, and near mangrove swamps—shepherds and others exposed to the miasms got decided tertians and, after long exposure, became affected to such an extent as to be unfitted for work and perhaps unable to remain. This paludal affection is known in Queensland as "*fever and ague*," or more familiarly as "*the shakes*." From all I can learn it is regarded without much dread and is looked upon as one of the chance ailments and incidents of bush life. The idea of danger is not associated with it, and in fact it would appear to be a simple ague, rather more intense in its first effects and more serious in its after results than the ague of Lincolnshire, but not nearly so formidable as the jungle fever of India or the vegetable malarious fevers of other tropical countries. There would seem to be no reason to suppose that, so far, the disease has been complicated with germs derived from an animal source. What settlement, or civilisation, may do, remains to be seen; but up to the present time, the remittent germs arising from the putrescence of animal organic matter have not been added to those of ague from the decomposition of vegetable matter. The so-called "fever and ague" of Queensland has not yet been converted by man into remittent.*

255. Why New South Wales, Victoria, South Australia and West Australia should have an exemption from ague is by no means clear. In many parts of all these colonies the conditions seem to meet, and yet there is something wanting, or there is

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* Since writing this, I have fallen in with the following acute remarks in Mr. Clement Hodgkinson's interesting work *Australia from Port Macquarie to Moreton Bay*, published in 1845.

"There are many inexplicable causes which produce wonderful diversity of
" climate. Thus, if I were called upon to judge from analogy, I should have no
" hesitation in saying that Australia was a most unhealthy country for Europeans;
" for the estuaries of its rivers, its creeks, salt-water inlets and mud flats, abound
" in mangroves, which have been considered by the best authorities the chief
" cause of the unequalled unhealthiness of the rivers on the coast of Western
" Africa. Again, there are in Australia an infinite number of tea-tree morasses,
" and reedy swamps, covered with stagnant water and rank vegetation; and the
" changes in the temperature, between day and night are probably greater in
" Australia than in any other country, and are also very sudden. Nevertheless,
" the experience of upwards of half a century has now ascertained that no
" country in the world is more exempt from all that class of disorders which
" originate in impure air, and deleterious miasma, than Australia. Indeed
" when I informed some persons in Sydney a few years ago, that ague was
" prevalent at the lower part of the MacLeay river, I was listened to with great
" incredulity, it seemed to them so totally incompatible with the climate of the
" colony; yet the reader will not wonder that cases of ague should occur at the
" MacLeay, for besides the mangrove mud flats at its mouth, there are, on its
" banks, at least, 60,000 acres of stagnant swamps covered with high reeds and
" water; and the decomposition constantly going on in the dense mass of
" vegetation on the alluvial lands, must also evolve a great quantity of noxious
" gases.

"Notwithstanding these obvious causes of impure exhalations, and the greater
" heat of the climate. the ague at the MacLeay river is much milder than in the
" fenny counties of England; the cold fit occurs every other day, but is seldom so

something present which precludes the formation of the ague-plant. I am disposed to think it is owing to the dryness almost invariably associated with the heat of the country. It appears that marsh lands when perfectly dry, or when covered with water, are alike incapable of developing the vegetation requisite to produce intermittent, and that it is during the process of drying up that the fungi are produced which convert the mists and fogs emanating from marshes into aguish malaria. From what takes place in Australia, it would seem that no sooner is a swamp or a piece of wet land deprived of its covering of water, than the exposed surface of moist mud or ground commences to dry up at once; or, where that is precluded, the moisture exhaling does not hang round the spot in the shape of reek or vapour, but is immediately taken up by the intensely desiccated atmosphere and dispersed. Except in fern-tree gullies at the foot of high ranges, or in narrow gorges into which the sun cannot penetrate, damp steamy air is unknown. That degree of humectation, therefore, which is required for the production and maintenance of the ague-plant is not found in these colonies; or, if the vegetation be produced in some neighbourhoods, there are no fogs or mists sufficiently dense to sustain the sporules and other parts of the plant—which is highly improbable. If the mildew on decaying vegetable matter existed in the vicinity of swamps, there seems no reason why it should not make itself felt here as well as in Queensland: so that the inference is that it does not exist. In any case, and whatever explanation may be forthcoming, there is the fact that there is no indigenous ague in the countries mentioned. The intermittent portion of remittent fever is therefore absent.†

256. As one essential condition of remittent is wanting here, I am driven to Queensland to illustrate my point. Why has it

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" severe as to prevent a man from attending to his daily avocations. Change of " air, and sulphate of quinine, remove the ague directly, but it is liable to return " by fresh exposure to the causes which produce it. Although I have resided " upwards of four years at the MacLeay river, I have never known there a " single instance in which ague has been attended, even in bad constitutions, " with serious symptoms of an inflammatory or typhoidal character."

There is a slight discrepancy between Mr. Hodgkinson's account of the ague and that which I have received from settlers, and learned from other sources. It may be pointed out, however, that Mr. Hodgkinson's observation of the disease was made more than 30 years ago and before Queensland was opened up, and was confined chiefly to one aguish locality. More enlarged experience would appear to have shown that in some districts the ague-plant acquires more intensity of principle; though it is a moot point. People used to die of tertians and quotidians in England at last in the swampy olden time. And from what I gather they do the same in some parts of Queensland [*olim* Moreton Bay] at the present day. The difference therefore between the English fen poison and the Queensland swamp poison is not very great, as regards virulence, in all probability. But the most interesting point in Mr. Hodgkinson's remarks is that he enables one to determine that the ague of Queensland is dissociated from remittent fever—or was rather, when he wrote. It is perfectly clear that the animal element had not mingled with the vegetable. The reference to the absence of typhoidal symptoms is shrewd and graphic and shows that the writer could distinguish between simple ague and ague complicated with typhoid germs.

† The hypothesis of the gum-tree destroying the ague germs is inefficient. For the eucalyptus is found in Queensland.

happened that in the tropical parts of Australia ague has not been known to be aggravated by any one form of the dangerous, and frequently very fatal disease, called remittent? How is it that the malaria which has efficiently conveyed the intermittent paludal germs has been entirely free from the "putrid" germs? Why should the tropical ague of New Holland never be associated with other deadly epidemical disorders, either amongst the European population, or amongst the aboriginals? I have gone carefully through the narratives of all the explorers in the North, and in not one of them is there any reference to the slightest touch of remittent, either amongst the party of whites, or in any of the various tribes through which they passed. There is no mention anywhere of a black fellow suffering from dysentery, cholera, remittent, ague, or fever of any kind or description—except small-pox. In the very earliest period of the settlement in New South Wales, Collins, White, and others, give an account of a peculiar kind of small-pox which had recently swept through all the tribes near Sydney and was still lingering among the few individuals left alive. I have called it peculiar, because although there was some intercourse between the convicts and the tribes affected, and although some of the blacks suffering from the disease were taken into the hospital near Sydney, the exanthem did not extend to the white population in any one instance—either then or subsequently. For the malady was found raging in the interior by Capt. Sturt forty years afterwards. With the exception of this terrible disease there is no allusion by any one of the tropical, or other, explorers, to a fever of any sort among the aboriginals—even in those swampy parts where remittents might have been calculated on almost to a certainty in any other region of the world. Anyone who considers the description of the features of the country given by Burke and Wills as they neared the coast; or of that passed through in the progress of Leichhardt along the shore of Torres Straits when he was making for Port Essington; or the account more recently given in the interesting and spirited little work of the Jardine Brothers of their trip with cattle across and up Yorke's Peninsula; any one who reflects on these subjects must be struck with the singular absence of all evidence of miasm or malaria from such scenes. Indeed they all seem to have been free not only from remittent, but many of them even from intermittent. If the reader will compare the experiences of travellers in most other parts of the world with those of explorers in this huge island, he will soon perceive the marvellous difference between them as to danger on the score of disease. The chief and almost the only peril of the bushman here is scurvy—or starvation. With proper food a man may live in any part of Australia in the open air at any period of the year in the most perfect health—as witness the aborigines, who knew no epidemics before the small-pox.

257. Can any philosopher resolve me this problem on any of the known or recognised theories or hypotheses of malaria? Can this salubrity of Australia be made to fit in with any of the generally-

received doctrines of the causation of zymotic or infectious diseases? Why should Australia be free from remittent—seeing that one half of the malady is present in some regions? There are no views extant which appear to me exactly to meet this unique case of an enormous stretch of ground over the whole surface of which the white man has found but one form of malaria. I neither find that the thing itself has been expressly explained, nor that any explanation which has been given with reference to the occurrence of the malarious diseases of other countries will adapt itself readily to the absence of malarious diseases from this country. I will, therefore, submit an explanation based upon the peculiar views herein set forth as to the origin of the deadly plagues of the world. In the first place the remarkable general anhydrous condition of the atmosphere before alluded to, may have something to do with arresting decomposition and thus preventing some of the ordinary results of decay. This almost universal dryness of the air has its due effect no doubt over a considerable extent of country; but it fails in the districts in which ague exists. There at all events the moisture is sufficient to produce the ague miasm, and it may be assumed that it would also be sufficient to produce the other half of remittent, if the material were there. The inference then from the non-production of the malaria which causes remittent, is that the material—the animal organic matter—is not there. This is simple enough, but the more complex part of the problem—that which relates to the absence of animal matter—remains. The hypothesis which has been submitted of the causation of remittent, is that it is the result of a fungus developed on excrementitious matter, when the germs from that fungus are mingled in air or water or both with ague germs. It was also stated as a part of the proposition that a very large proportion of the excrementitious matter supplying the fungi was derived from man and monkeys; and that although the vegetation might spread to and subsist upon the excrementitious matter of other animals, and indeed upon other organic matter, non-excrementitious, under certain favouring conditions, yet in most cases it was originally started in existence on the excrement of man or monkeys. In fine remittent was believed to be caused principally by the germs of typhoid fever. It now devolves upon me to show how it is that the ague districts of Australia have not been poisoned by men or animals—how it is that the excrement of the native savage has never effectively developed that fungus which is assumed to be the efficient cause of remittent. I admit that the subject was one of doubt and perplexity, until I accidentally discovered the curious explanation of the mystery. It was difficult to understand how it should have happened, that the excretions of the blacks should never have been observed to have produced any one of the deleterious results I ascribe to fœcal matter in all parts of the world. For even taking into consideration that the country was very sparsely populated, yet there was no getting over the fact that at stated periods the tribes would meet in hundreds for the "corrobary," either on a creek, or near some permanent water-hole or lagoon, when there would necessarily be accumulations of excreta. But besides these occasional temporary gatherings of all the scattered members of a tribe, there were the accounts of the discoverer of the Murray, and of the "overlanders;" from which it was evident that whatever might be the scarcity and nomadic character of the population in the far interior, there were considerable numbers of blacks living permanently on the banks of the river. The narrative of Sturt's entrance of the Murray from the Murrumbidgee, and of his perilous voyage to Lake Alexandrina and back, shows conclusively that some thousands of aboriginals must have been congregated here and there within a small area. Here then was abundance of material for the production of local malaria; and it was puzzling in the extreme to divine how it was that its formation had been obviated, or prevented, among Australian savages.

258. In estimating the amount and kind of animal matter in the ague districts of New Holland, the agency of the monkey may be disposed of at once—that animal not being indigenous to the country. Then with regard to the excreta of other animals it may be observed that the fauna of Australia is singularly deficient in large quadrupeds. There are none of the immense herds of game of all kinds found in other lands. The poorness of the vegetation may perhaps account for the absence of grass feeders of larger size than the kangaroo. However there is the fact. The bush is almost tenantless except by the marsupial tribes and the wild dog—unless on the north and north-eastern coasts where it is said there are buffalo. These, however, are confined to restricted areas and do not affect the present proposition. Although the kangaroo is very thinly distributed throughout the country, which, for hundreds of miles, perhaps, shows no sign or trace of them to the traveller, yet in times of great drought all the outlying animals are forced to concentrate on those portions of country where there is to be found a permanent water-suppply. During ordinary summers many of the water-holes to which marsupials, dingoes, emus, and birds resort, run dry, and all living things have to fall back on the larger lagoons. This involves more or less water-pollution, both from dung and from the decomposition of those beasts and birds which only reach water perhaps in time to die in it. But when two or three successive dry seasons occur, there is a still larger amount of animal concentration and consequent pollution of the water. It moreover happens that at these periods the blacks resort to these water-holes for the double purpose of procuring game and water. So that although the totality of excrementitious matters finding their way into water, or into positions where they would have the necessary conditions, or all but one, for developing fungi, may be small, in comparison with the wide extent of country, yet there would be local collections of organic material on such occasions as these, quite sufficient in amount to poison both air and water and cause typhoid or remittent among the natives—if one

of two things occurred. As typhoid is unknown among the natives, neither of these things can occur. Neither do the dung of the living and the bodies of the dead animals in and around these water-holes originate the typhoid germs; although the required conditions would seem to be all present;—nor do the excreta of the natives evolve the germs;—although they must be assumed to be in the neighbourhood. With regard to dung and dead animals, I conclude that whatever inherent property they may have in other countries, they are incapable of eliciting the typhoid, or any allied, germs, in this country. And with regard to human excreta two conclusions offer themselves:—one that the fungoid hypothesis breaks down; and the other that the excrement-disposal system of the aboriginal not only differs from that of most other black and many white nations, but also differs materially from that of a British population in the Australian bush—as seen notably at every new gold-field.

259. I was for a long time uncertain which of these *ultimata* I should be forced finally to accept. But as the point arose in the consideration of dysentery—the absence of which disease from the native encampments being in such strong contrast to the epidemics in the white encampments at the diggings—I will refer the reader to the account of the mode in which the natives of this country deal with their excreta which will be found in the concluding remarks on dysentery. [310.] It is sufficient to say here that these savages adopt the Mosaic law; by which wholesome procedure they not only escape the noisomeness of the Hindoo custom and keep the air round their camps sweet and pure, but they thereby avoid all the diseases which every other people, except the Japanese, entail upon themselves. This practical earth-closet system effectually excludes typhoid and of course remittent fever. It is the explanation I offer of the perfect freedom of this country from these diseases until the white man came. He has shown conclusively enough that typhoid malaria is quite possible here; and that its former absence was not due to the fact that it could not be produced. He has proved that when the conditions are brought together by civilisation, the climate is quite competent to generate "Colonial," "gastric" and "bilious" fevers —all variations of typhoid and all admitting of being converted into remittent; as will probably be seen when Queensland becomes more settled, or as civilisation extends.

260. The immunity of New Holland up to a recent period from malaria, I regard as a striking proof of the soundness of the view that what is commonly called unhealthiness of climate is a matter altogether dependent on man himself—a thing entirely apart from the natural condition of a country. This may be shown even yet more conclusively a few years hence, should the introduction of coolies into the sugar plantations of Queensland be carried to the same extent as in the Mauritius. If the "foreign labour" market with the South Sea Islands be closed, and if the planters have to seek for a larger labour supply in India and China, as seems not

unlikely, it is by no means improbable that Queensland will exhibit an analogous state of things to that in the Mauritius. For it is not to be supposed that a people will take warning by the fate of another people. The colonists in the North, therefore, will probably furnish another illustration of the fact that man makes his own climate. The result cannot be so terribly disastrous as in the Mauritius for many reasons; yet notwithstanding the difference in the physical configuration and atmospherical conditions of the two countries, and in the distribution of the population, it will be quite sufficiently marked to show that surface pollution of a country cannot go on largely and for any length of time without producing malarious diseases, where malarious diseases were before unknown. But Australia is not the only country which confirms the proposition that fœcal contamination governs all malarias—but the one. Wherever the enquirer may turn, he will find it, as I believe, to be an absolute rule, that the unhealthiness of a climate, or rather the insalubrity of a country, is in direct relation to the presence of man and to the mode of his excrement-disposal. In a few days one can go from Shanghai—as pestiferous a place as most—to Nagasaki—a town perfectly free from infection—unless perhaps in the lately more civilised European quarter. Even in India there are spots beyond the haunts of men where the climate is as salubrious as that of any tropical country to the European. In Ceylon the elephant hunter may roam for years in perfect safety, if only he steers clear of the native element. So in Java—one of the most wholesome and charming of countries, if you can get out of the artificially formed malaria surrounding collections of the people. And here I might point to the vast improvement the Dutch have effected in the condition of Batavia;—a city which has been redeemed. Fifty or sixty years ago it was in a deplorable state as regards fevers and dysentery, as may be seen on reference to a paper in the *Edinburgh Medical Journal* of 1842 by Mr. Prior. But the Dutch have both disinfected and deinfected the city and have completely altered it, so that living there is not only possible, but agreeable. I do not know whether the shrewd Hollanders have got a wrinkle from Japan, with which country they have had commercial relations for so long, or whether their natural cleanliness as a nation has induced them to effect such a marvellous change in Batavia; but there is the fact that the place is far more wholesome and pleasant than most tropical cities. In the paper by Mr. Prior just alluded to there is a curious reason suggested for the prevalence of malignant dysentery in Batavia, which he regarded as strange considering that it was so much more virulent there than in other parts of Java. It was supposed to be in some way connected with volcanic vapours and disturbances! But another suggestion thrown out bears directly upon the point as to man's agency in the production of malaria. Mr. Prior urges the advisableness of transferring all the dysentery cases to some *uninhabited island* in those seas—not only a most practical measure, but a notion showing that he had a very clear perception that Malay influence in a country was not beneficial to health. To go to Africa. The climate is excellent wherever one gets beyond the reach of man's pollution. In Algeria the French troops got the regular camp diseases only when they halted. The rapid advance on Magdala and the equally rapid and successful march back from the interior was about as instructive a lesson upon this point as the world ever had—though the world has been rather slow to read it aright. Everyone will recollect the questions that arose at the time as to the prospects of the expedition in the matter of fevers, and malaria, and the predictions of some that there would be awkward results on this head if the force were entangled or delayed in the country, until the unhealthy or sickly season should arrive. When the work was done and the troops re-embarked and all accomplished with fewer deaths than would have occurred in the same numbers during the same time in Liverpool, it was looked upon as a marvellous piece of good fortune that we should have hit off just precisely that season in Africa when the thing was possible or practicable. It does not seem to have occurred to any one to ask whether the country through which we passed is necessarily unhealthy at any season; or whether, supposing the whole force had been detained at any one point upon the line of march for the same time that was occupied in the country altogether, the outcome would have been equally satisfactory. But if any one will work out the problem of the exceptional escape of this invading army from disease, he will, I think, soon begin to doubt the existence of malaria in that region of Africa traversed by our troops at any period of the year. That is of malaria dependent on natural conditions of soil and climate. Every soldier knows well enough the perils that he runs in an encampment. He learns by experience that so long as he keeps on the move and stays no more than a few days in a place, he is safe from dysentery and fevers, but that if he is compelled to prolong his stay the whole tribe of camp diseases is sure to break out. And what is this but a veritable practical fabrication of a malaria which did not previously exist on the spot? What better exemplification can be given of a purely artificial product commonly ascribed to a natural process? And carrying out the idea further, what are Indian villages but so many permanent encampments retaining always the temporary system of excrement-disposal in vogue in armies while in the field, or on active service in a foreign country? Assuming that the route of the British force sent to Theodore's capital had lain through a friendly region dotted every few miles with villages in every respect like the Indian; or in other words, supposing the ground to have been saturated with stercoraceous material to the same extent as in India; is it probable that the same tale would have had to be told of the expedition? Would there not have been malarious influences at work? And would not the country have been described as naturally unhealthy? But I shall have more to say touching the escape of Sir Robert Napier's force from dysentery, which is unique in its way and elucidates my theory of the causation of the flux.

261. At the Cape also there is another admirable instance of the benignity of climate where man's influence is excluded. But even at the Cape, healthful as it is under natural conditions, it has been seen by Dr. Lichtenstein's account [17 and 18] what terrible epidemics may be brought about by human agency. Any one who goes through Livingstone's Travels in South Africa, will be struck with the same evidence of the healthfulness of the interior of the country just in proportion to the interference of the races. The country of the Bakwains is enthusiastically described. "Mr. "Oswell thought this climate much superior to that of Peru, as "far as pleasure is concerned;" and Livingstone himself says—"Were it not for the great expense of such a trip, I should have "no hesitation in recommending the borders of the Kalahari "Desert as admirably suited for all patients having pulmonary "complaints." It was only when he travelled into the swampy regions that he was attacked with ague; and the number of attacks he had shows them to have been almost, if not entirely, free from any remittent complication. On reaching the eastern coast he found traces of remittent evident enough at the settlements. The account of Kilimane is highly suggestive. No traveller, however, is more instructive in all these matters than Burton. The reader looking through his pages with an eye to this special question will find abundant material. He is admirably full always upon points of domestic economy, whether in Zanzibar, at the Salt Lake City, or at the City of the Faithful. If one looks to America, either North or South, the proofs as to the true origin of malarias accumulate. Leaving marsh miasms out of the question, there is not one of the infectious diseases named that may not be followed easily to its source in human contamination of the soil. I cannot, however, write a history of malaria. It would be to undertake the History of Civilisation. I must content myself with stating that there is no proposition of the literal and perfect accuracy of which I feel more assured, than that every island and every main land was absolutely free from malaria until man created it by his own act. As a corollary, every nation that has blindly empoisoned its own originally pure and wholesome atmosphere may, by enlightened measures, restore it to the untainted condition in which it found it. There is no physical, or climatic, reason, though there may be social and economical reasons, why England and all Europe should not be as healthy and as exempt from epidemical disorders, as Japan is, and as New Holland, until we took possession of it, was.

ON

DISINFECTION AND DEINFECTION

IN RELATION TO

TYPHUS FEVER.

262. The principle of the disinfection and deinfection for typhus can be established only upon the one basis—that of a knowledge of the causation of the disease. And though I entertain no doubt myself as to the implication of fœcal matter in its causation, I do not see my way quite so clearly here as in the causation of typhoid fever. While I consider that the cause of typhoid has been brought home, with so much probability as to amount almost to a certainty, to a mildew starting in existence on excrement—[though probably capable of lateral extension to other substrata]—I am prepared to admit that the chain of evidence to be submitted as to typhus is not so strong. The conditions under which typhus is generated are markedly distinct in some respects from those by which typhoid is developed. And yet there is such a general resemblance in some other of the conditions of the two diseases, as to justify the conclusion that they are, in all likelihood, closely related as to causation. Many of the circumstances surrounding typhus point to excrement as the origin of the disease, and to a mildew on excrement as the means of infection; although it is difficult to apprehend without more knowledge than is at present available, the nature of the conditions which determine some of the vegetation to the typhus, some to the typhoid, mildew. For though typhus is more commonly met with apart from typhoid, yet it is by no means uncommon to find them flourishing side by side, especially in camps, besieged towns, and crowded poverty-stricken localities with an excrement-sodden soil; in which places, indeed, they overlap and complicate and confound symptoms. It is easily enough to be understood that two distinct forms of vegetation should occur on two similar kinds of soil, in contiguity, but under different conditions as to fallow, manure, and so forth. The difficulty is to ascertain what the conditions are that will result in a given vegetation. I have a clear conception in my own mind as to the conditions for the typhoid mildew, but I cannot altogether satisfy myself as to those of the mildew of typhus.

263. The contagious nature of typhus is a disturbing element in all calculations as to its causation. This ingredient of uncertainty is absent from considerations as to the origin of typhoid, dysentery,

cholera, ague, and remittent, none of which are contagious:—that is by mere contact with the body, or by being in the atmosphere, of an infected patient. But typhus is supposed to be contagious in the sense that small-pox is contagious, though not nearly to the same degree. It is believed that particles of matter are given off from the bodies of typhus patients, and that these particles are capable of inducing typhus in those persons into whose systems they are introduced. Nearly all the authorities concur in this principle of *fomites*, or a *contagium vivum*, emanating from typhus cases. The question of origin is, therefore, always liable to the intrusion of the doubt as to whether given instances of the disease may not have resulted from contamination, rather than that they have been generated *de novo*. But notwithstanding that this tends to complicate the question, it cannot prevent the discovery of the typhus germ, when investigators shall set about it with a method made more exact by a larger knowledge of the mucology of disease. Pathologists will sooner or later have to recognise the fact that the vegetable parasites of the human body are entities, and not brain creations of the German school. Philosophical Germans have not been working so assiduously in this field, without a clear practical result ahead of their labours. They have not been frittering away time and energy on scientific toys, or in dreamy speculations leading to nothing. While the rest of the world have been looking on with a half sneer at their hypothetical disease germs, incredulous as to any tangible good that could come out of these insufferably minute and peddling investigations ending only in a *Mucor*, or a *Zooglœa*, these shrewd hard-headed men have been straining every nerve to connect the cryptogams with their primary substrata. For they have evidently had an acute perception of the fact that to do this successfully, would be to revolutionise hygiene and convert the sanitary art into a science. Hitherto their efforts have been concentrated on the ingesta of the human body, principally, and they have not yet turned their attention to the egesta. When they do get hold of the idea that excreta are the source of most zymotic diseases, they will lose no time in putting it to the proof. And I predict that Germany will be one of the first—if not the first—of the nations to be deinfected.

264. As the *noscitur a sociis* principle would seem to apply to many of the disease germs, I should certainly expect to find that excreta are inseparably connected with the origin of typhus. Some human element at all events is deeply concerned in the causation, as all agree. And there is none more likely than excrement, that I can see at present. Looking at the conditions which appear to favour the development of the disease—crowding in cold climates especially—I offer it as a purely speculative notion that a de-oxygenated atmosphere of a certain temperature, though it might preclude the evolution of the typhoid mildew, may allow of the generation of the typhus mildew; which may be in the form of a light dry and hardly perceptible mould. There can be no

doubt that one of the results of ill-ventilated, over-crowded dwellings, is that the air in them is loaded with nitrogenous matter. This is the cause of what is commonly known as the *"poor smell"* of such places; and the length of time to which this "poor smell" clings, indicates that the organic emanations from the bodies of persons effect a lodgment somewhere. All this has been shown exactly, I believe, and the incrustations and accretions formed by these inorganic particles have been carefully examined; but I cannot lay my hand on the account at this moment. Assuming, however, that these accumulations of animal matter occur, it is by no means unlikely that the sporules of the typhus plant may light on them and may germinate and subsist, though perhaps in a modified, or imperfect, form. In this way I can understand how the "lazarette" or "gaol" fevers, and the typhus of low, crowded haunts, may be propagated when once the germs have started on their own specific substratum—whatever it may be. For it is perfectly clear that crowding and bad ventilation in cold or temperate latitudes, do not furnish all the factors of the typhus poison. The organic matter given off from the surface of the body, or from the lungs, is incapable of originating it. Something more is required. For not only are there innumerable instances where all these factors have been present and typhus has been absent, but there are many recorded cases where typhus has occurred, sporadically, when these factors could not have been concerned in the causation. In fact, crowd-poisoning alone cannot cause typhus; and there is an unknown element to be yet determined. The astute Niemeyer believes that this unknown element, whatever it is, results in a *"low vegetable organism,"* which he calls the *"typhus germ."* If he is right in his deduction—and I can see no other conclusion—a mould, or mildew, on human excrement, appears the most likely shape in which to find the "low vegetable organism." If the objection be taken that the excreta can hardly play so many parts as have been assigned to them, and that they are unlikely to give rise to such a variety of vegetable forms, one has but to point to the numerous species of plants springing out of the same plot of ground, at the same and at different periods, in the course of one year. Or there is the more direct case in point of the cryptogams, already made out by mucologists, on cow-dung. Nine separate and distinct fungi, at least, have been found existing on that substratum. The parasites on human excrement not having as yet received any great attention from observers, it is impossible to say how many may be found eventually. But by ordinary analogical processes of reasoning—by inferences drawn from observations in adjoining fields—it seems a fair and reasonable deduction that the excreta of man should be affected by as many fungi as the excreta of a grass-feeding animal. I assume more, and I think the assumption will be found warranted by the result of investigation. If again it be difficult to understand how excrement can be the substratum of the typhus germ, when it is generated under circumstances where the connection of fœcal matter with the disease would seem to be precluded—as in highly civilised life—in palaces even;—I answer that the carelessness, or negligence, of a servant, may afford a very sufficient explanation in some rare cases of infection, but that a more probable and more common one may be found in the water-closet system.

265. One strong reason for connecting typhus with fœcal matter is the fact of its occurrence in camps, where it is often found associated with dysentery and typhoid. The three diseases indeed seem to be intimately related as to origin. Typhus and typhoid fevers may, and do undoubtedly, occur altogether independently of dysentery—as in England at the present day. But the converse does not hold good: for an epidemic of dysentery has never occurred without one or other of them. Isolated cases of dysentery may be heard of without either of the two fevers; yet there is not, in the whole history of dysentery, an account of an epidemic outbreak of the flux without its accompanying typhus or typhoid. Every author has, either specially or incidentally, referred to febrile affections, as coincident with dysentery epidemics everywhere. This appears to be a constant:—*ubi* dysentery, *ibi* typhus, or typhoid, or both. The first inference I draw from this alliance between the affections is, that all three have a common base. (I conclude excrement to be the base.) The second inference is that whatever divergence there may be in the conditions for the evolution of the three germs, the germs themselves are *ejusdem generis* and cause infection in a similar way. For though the after-extension of typhus differs materially from the mode by which typhoid and dysentery are propagated, yet there is no reason to believe that the introduction of the germs varies in the first instance. I therefore assume that the infective principle of typhus is some form of mildew analogous to the mildews of typhoid and dysentery. This supposition would appear to involve the proposition that the three mildews may be developed by and may occupy the same, or an adjacent, collection of excrementitious matter at one and the same time. Yet this does not follow of necessity from the hypothesis. Indeed while I consider that typhoid and dysentery fungi may exist side by side on the same mass of fœces, or on fœces left exposed in the same neighbourhood and deposited at different periods, yet I suspect that the fungus of typhus cannot be formed in the open air under similar conditions to the other fungi. I think moreover that some degree of crowd poisoning is almost essential to the propagation of typhus—some such crowding as frequently takes place in military hospitals after a battle, or on board ships with sick or wounded troops or sailors. Where typhus is associated with dysentery and typhoid there is generally some history of crowding—if not invariably. The case of the force embarked at Corunna [146] is a good illustrative one; but the instances are innumerable. The fact that typhus was almost unknown among the Germans in the late Franco-Prussian war, appears to me to be one of extreme importance to the subject of causation. Dr. Wibel, who was attached to the Prussian army,

remarks in his letter to Dr. Hermann Weber, published in the *Lancet* in November 1870, that among the troops in the hospital at Nancy, there was no typhus in an exanthematous form; and that the line between it and typhoid was difficult to draw. Dysentery and typhoid were there without question, but from the mode in which Dr. Wibel alludes to typhus, the impression conveyed is that he was somewhat doubtful as to its actual presence in the hospital. The diagnosis between it and typhoid was by no means clear. If we turn from the hospital at Nancy, where the arrangements were all in excellent working order, evidently, and the system of nurses, food-supply and so forth as complete as possible; if we turn from this scene of regularity to the army Bourbaki poured into Switzerland, the contrast as to the camp diseases is most marked. I have not seen any accurate account of these diseases and therefore precision in speaking of them is precluded. From the journals of the day, however, one may form some rough conclusions as to condition of affairs in the army. The disorganised state of Bourbaki's divition entailed not only the usual consequences among the weakened, wounded troops themselves, but the implication of the Swiss population in their camp disorders. Their fevers spread, and though the journals speak of typhus and typhoid indiscriminately, and though it may be difficult to say whether true exanthematous typhus was there, it is at all events clear that the conditions for its development were there; and I think it may be safely assumed the disease was present. In any case it is established beyond doubt that the sick and wounded of a retreating army have been affected with typhus. That the fever among the troops at Plymouth [146] was typhus, and not typhoid fever, may be assumed from the fact, mentioned by Mr. Hooper in his account, that one of the medical men in attendance on the sick in hospital caught it and died. This is not conclusive evidence of course, but coupled with the other corroborative testimony, this instance of spread by apparent contagion is strong presumptive proof amounting almost to a certainty. This point being conceded—and it is one upon which all writers seem agreed—there should be no great difficulty, I think, in seeing how it was that the Germans in the hospital at Nancy escaped typhus, or had typhus in such an undeveloped, or uncharacteristic, form that Dr. Wibel would appear to have had some doubts as to its being typhus at all. The conditions surrounding the Prussians in hospital seem to me to have been very nearly as good as those to be found in most hospitals in civil life. They had excellent nursing, an appropriate *cuisine*, constant and effective medical attendance, and no doubt the requisite cubic area for breathing in. Add to all this, order, management, and that extreme cleanliness which modern treatment of the sick exacts and of which modern medical science fully appreciates the necessity, though it may not yet precisely apprehend the object, and there are just those exceptional conditions (for an hospital in a hostile country), which would be calculated to exclude typhus. One of the essential factors was wanting.

266. If instead of being in this improvised, but commodious, hospital at Nancy, the same German sick and wounded had been huddled together on the floor of some small building, where cleanliness was not to have been achieved and the excreta of some of the dying would thus probably have been left for days, then they might have generated true exanthematous typhus. But placed as they were, it would have been strange had the disease appeared among them. For camp typhus is not generated outside hospitals, as are camp dysentery and camp typhoid. Cases of typhus are not brought into hospital, as cases of typhoid and dysentery are—except perhaps in some peculiar cases arising out of the chances of war—such as the shutting up of a large force in a small fortress. Typhus, therefore, is rarely introduced into the military hospitals of an army in a foreign country. The disease appears to be so closely related to confined habitations or crowding in some shape, that, unless contagion be brought into play, it never occurs when men are actively engaged in field operations and living in tents. Men do not fall ill with typhus, as they do with typhoid and dysentery, under such circumstances. The most common way in which typhus is induced is after a severe engagement, when the wounded have to be got under cover and treated as well as the position of affairs will admit. And it is then that all the means and appliances devised fail to alleviate the horrors of war in many instances. No adequate provision can always be made against crowding and fœcal impurity—especially under disaster. Even if a retreating force is able to carry off its own wounded, the chances are, when the numbers are very large, they have to be so disposed of that typhus is generated almost to a certainty. When men in a helpless state are closely packed together, in temperate or cold countries, without due care and attention being directed to an early removal of alvine discharges from the bedding or clothing, the factors and momenta for the production of the low organisms of typhus would seem to be almost invariably brought into operation. The presence of fœcal matter appears to me to be a condition required. On this assumption it may be understood how crowding may take place to a very great extent where persons have the means, the will, and the power of getting rid of their excreta, and the specific germ of typhus is not a product. But if individuals among the mass become disabled from any cause and are incapable of removing their discharges and the others do not remove them, the evolution of the germ is imminent, if not a constant. This is I take it the explanation of "ship fever." The closeness and highly nitrogenised state of the air below does not breed fever so long as the men are able to go on deck. The quarters may be dirty and foul smelling and what persons, accustomed to breathe a purer air, would consider stifling and sickening and positively unwholesome. And so they are doubtless. They have their deleterious effects upon nutrition without question. But these offensive holes are slept in by the crew voyage after voyage without typhus breaking out. Let all hands get scurvy, or dysentery,

however, or let one of their number get injured so that he cannot cleanse himself, and typhus is induced and the crew affected. This is the history of most cases of "ship fever;" and it is difficult to assign any other cause than the one for the outbreak of the malady. It will have been observed that I have hitherto set aside the view of Dr. Murchison that the specific poison of typhus is the result of over-crowding and want. With every respect for the opinions of so eminent an authority, I must say I do not think he has established his position, though he has made out a presentable case. I suppose I am bound by my hypothesis to show wherein I consider his argument defective. His views, then, briefly described, are that the infective agent of typhus is a specific substance; and that this substance, leaving *fomites* out of the question, is generated *de novo* by factors supplied by crowd-poisoning, *plus* want. Dr. Murchison concedes the point that typhus occurs where want is not present, and he of course recognises the fact that over-crowding may take place without the production of the specific poison. Now on my view of the mode of evolution of a specific poison, the factors of the poison are just as specific as the poison. If therefore it has been clearly shown that the typhus poison originated *de novo* when want was not one of the factors, on one occasion only, it follows that want cannot have been a necessary factor on any other occasion. It appears to me impossible to conceive a specific product without specific factors. As a logical consequence a specific product cannot result in the absence of one of its specific factors. Want once excluded, shows it can never have been included, as a factor of typhus poison. But independently of this view of the law which governs specific products, which may not be accepted, let it be conceded that this objection to want as a factor does not obtain. Let it be granted that want has never been absent when typhus has been generated. Let want be taken on its own merits as a probable element in the typhus poison. And as questions of want, exposure to cold, mental depression, and so forth, are constantly cropping out of all speculations as to the causation of specific diseases, it may be as well to clear the ground a little and try to get at some definite notion as to what is really meant, when things relating to a person are spoken of as being probably concerned in the production of a substance outside that person. How does want contribute a factor to the construction of the typhus germ? That germ is admittedly a substance. To show that want is concerned in the formation of that substance, it has to be shown either that it supplies matter, or brings certain forces, or momenta, to bear on that substance. One of these two things must be shown to connect it with the formation of the substance. Which is it? Does want cause the emanation from the body of particles of matter which go to form part of the infective substance? Or does want by some occult means induce certain changes, or evolve certain conditions, in matter external to the body, so as to bring about the conversion of that matter into the specific poison of typhus? Before these questions can be answered, however, it is essential to know what want is. I apprehend that Dr. Murchison uses the word want to imply insufficiency of food and clothing—defective as regards either quality, or quantity, or both—extending over a greater or less period of time. Inanition or slow starvation, in short, is the sense in which I take want—that form of want which Dr. Murchison considers to be implicated—as a factor—in the typhus poison. This point being fixed, let us consider first what are the visible, tangible, recognised, primary effects of slow starvation. The fat goes, the blood diminishes, the muscular and other structures waste and degenerate, and the person is said to fall off, or to lose flesh and weight. The degree of the starvation of course governs the amount of depravation of the body. The question now is how far an advanced stage, or any stage, of slow starvation, is concerned in the production of the typhus poison, when combined with over-crowding. What material does it add, or what influence does it exert on the material already there, but awaiting the influence to create the specific substance? First as to material added. In extreme cases of rapid starvation occurring from total deprivation of food, it is said, the exhalations from the lungs and the skin have a peculiar, fœtid odour, and the exudations from the skin are of a brownish colour—that is in the very last stages. Physiologists explain these fœtid emanations by the diversion of the urea compounds from their usual channels of exit. If the fact be as stated, and if the explanation be correct, it follows that the exhalations of a person in this condition contain a larger proportion of nitrogenous material than those of one in a normal state. But the history of these instances of pure starvation, apart from complications with crowd-poisoning, is not very clear, or full. There are very few recorded cases where downright starvation has taken place, in which the victims have had a sufficient supply of oxygenated air—a material point in this enquiry. However, let it be assumed that organisms in an advanced condition of absolute starvation exhale far more nitrogenous material than bodies in a natural condition. Granting this, it does not materially affect the question as to the emanations from persons undergoing gradual wasting from inanition, which is what I understand by want. Mere want is a very different thing from utter starvation; and indeed it is most unlikely that Dr. Murchison should have intended to imply the latter, seeing that it must be a very rare thing in England. The poor-law system almost precludes it. But the poor-law does not preclude inanition among certain classes at certain inclement seasons of the year, when they are temporarily thrown out of work, and when, pinched with the cold, they want more aliment and get less. At such times these classes—in spite of out-door relief—cannot procure sufficient material for oxidation or to repair waste. They are unable to maintain their systems, in fact, without drawing upon such a reserve fund of fat as they may have put by under more prosperous circumstances. By slow degrees their store decreases and in a few weeks they are shrunken, flabby and debilitated. They have now arrived at the position of being really in want, and it is undoubtedly among such people that typhus most commonly occurs when they are over-crowded in their dwellings.

267. While persons are undergoing the process of emaciation described, do the emanations from their bodies necessarily differ materially from the emanations of those in good condition? For my part I think not. Their condition has no analogy to that in the latter stages of starvation. There are no sufficient reasons to induce the belief that half-starved people exhale greatly different material from well-fed folks. And as regards the quantity of material exhaled, the probabilities are that the exhalations from the latter would outweigh those of the former. Indeed I strongly suspect that if the same individuals who, when emaciated, huddle together and produce crowd-poisoning, were to do precisely the same thing when properly nourished, they would produce the same, or even a greater, degree of crowd-poisoning in the same time. For the healthy system requires certainly as much oxygen as the debilitated system, and a full man will produce at least as much carbonic acid as an empty one. At least I believe so, but I will put this proposition to physiologists; for my physiological lore is not profound enough to enable me to speak by the card. Let one hundred persons in health be confined in an apartment with breathing space sufficient only for fifty, and let them be amply supplied with food. Let the same number of persons reduced by want below their standard of weight, but in all other respects the same as the healthy persons, be confined in a precisely similar apartment, and let them be supplied with food moderately. Let the excreta of both be carefully removed and let all other conditions be alike. It is required to say in which apartment at the end of a week there would be found the greater amount of crowd-poison—as shown by the tests used for determining the nitro-compounds in nosocomial air. My conclusion is—though I arrive at it by rough and inexact methods—that there would be a slower combustion, a less intense oxidation of tissues, in the bodies of the emaciated persons, and consequently (?) less ammoniacal vapour and less albuminoid or other nitrogenous material in the air of their apartment, than in the apartment of those who were the more robust at the commencement. A supposititious case is not an argument, but it serves to illustrate my meaning—which is this :—that want, as such, does not contribute any specific elements to typhus-forming materials, that plenty would not supply equally, or more abundantly. Unless it can be demonstrated that the exhalations of poorly-nourished persons contain some special matter not to be found in the exhalations of the well-nourished, I submit that Dr. Murchison's view lacks the support even of probability. For the fact that typhus occurs more frequently among the destitute classes may be an accidental circumstance, merely indicating that its factors are oftener brought together among them than among the other classes. It must be shown that the

rich do not breed typhus when submitted to the same conditions of over-crowding as the poor, before it can be admitted that want is a required factor. And not only has this not been shown, but the converse has been shown as in armies, jails, ships, &c.: and, moreover, it has been shown that typhus has originated when neither want nor crowding have been factors:—*a fortiori*, therefore, want cannot be a necessary factor of the typhus poison. Admitting that those afflicted with want and its concomitant, mental depression, contract this disease more readily and succumb to it more rapidly than those in good health, what has that to do with the causation of the disease? The same results accrue with regard to other diseases with the causation of which want is not connected as a factor. Want may be a "predisposing cause;" but a "predisposing cause" is a quality pertaining to the individual, and has no necessary connection that I can perceive with the causation of a particular infective substance formed apart from the individual. In fine, the only thing which appears to give an air of support to the doctrine that overcrowding and want will of themselves generate the typhus poison *de novo*, is the one fact that persons in want more frequently contract typhus than others. But a frequent is not a constant. And now with regard to any influence which want may have in the formation of the specific infective substance which is the agent of typhus. It is not necessary to use many words in dealing with this question. The only influences one need consider are of a physical nature. I know of no other means by which want can affect a thing than by adding to, or taking away, from it. Additions have been considered and subtractions may soon be disposed of. They all resolve themselves into the constituents of air which may be removed. Certain gaseous elements are taken from the atmosphere by persons in want; but as they would also be taken by persons in affluence, there is no special peculiarity of effect on things within the atmosphere. If want, therefore, neither contributes a thing *sui generis*, nor abstracts a specific thing, the infective thing is disconnected from want. Want and typhus, then, are associates, not relatives. The explanation which I submit of the frequency with which the poor are afflicted with typhus is in effect the same as that suggested with regard to "ship fever;" viz., that while crowding and poverty may coexist without typhus so long as the fœcal element is excluded, yet when some accidental intrusion of this material within crowded dwellings occurs—as from drunkenness, or from the habits of children, or from any of the denizens being bedridden from age, illness, or injury, and left without care or cleanliness—then typhus is probably generated. Nothing can well be more clear than that every want-beridden, crowd-infested, locality, does not engender typhus, unless it be that something else is required to engender it. As I concur with Niemeyer that that something is a "low vegetable organism," so I am unable to discover, throughout the whole range of organic matter, any substratum so likely to be the nidus of this "low vegetable organism," as excrement. Whether the hypothesis will bear strict analysis, or whether investigation will show that excrement is the base of the infective agent, I must leave—as I have had to leave many other things—to be determined by others. To return to that part of the argument from which this reference to Dr. Murchison's view has been a long digression—the affiliation of the three camp diseases, typhus, typhoid and dysentery.

268. Assuming the typhus mildew, I may epitomise the views I hold as to the conditions of origin of this trinity of hypothetical mildews on the one substratum. The *dysentery* mildew is formed only on fœces in the open air on the surface of the ground. The *typhoid* mildew is formed on fœces either on the surface, or below the surface; or on fœces mixed with earth, or other material, to an extent short of complete occlusion of the external air. The *typhus* mildew is formed only on fœces in places occupied by living organisms and, most frequently, under conditions producing what is known as crowd-poisoning. If this differentiation between the modes of origin of these three diseases be now accepted and afterwards confirmed, it will be easy to see the vast practical importance of a knowledge of causation in dealing with disinfection and deinfection. For it follows that the measures to be adopted for the destruction of the mildew, or to preclude its formation, must vary in each case. And it will be evident on reflection that the dysentery germ is the easiest to destroy, or to obviate, and the typhus germ the most difficult. The typhoid germ occupies a middle position. It involves more time, pains, labour and expense than the dysentery germ, but it is not so unmanageable as the typhus germ, which in many instances it is manifestly hopeless to calculate on destroying or preventing. So long as cold countries are afflicted with pauperism typhus will be intractable. And yet on the whole, taking the world throughout, the sum of lives lost by the typhoid germ will probably always far exceed the mortality from the typhus germ, or from the dysentery germ either—although the flux is supposed by writers to be the most deadly, as to numbers, of all diseases known. When, however, the dual causation of remittent fever is made out and the non-aguish principle relegated to the typhoid mildew, as seems to me probable, I think there will be no longer any question as to the most largely fatal of the zymotic affections of the world.

269. If it be difficult to see how two diseases like dysentery and typhus should ever meet together in one individual, supposing the hypothesis as to their mode of causation to be sound; viz.—that the one is induced by a germ developed in the open air, and the other by a germ brought forth only under precisely opposite conditions;—if this combination should appear at first blush subversive of the view taken, I submit that the facts and the hypothesis may be reconciled without the destruction of the latter. In the first place, are dysentery and typhus found in the same person at the same time? Although the concurrence must be rare and confined almost entirely to military and naval experiences in cold climates, yet that the two affections may meet in the same body, and do

actually come together, must be admitted. Theoretically there is no reason why they should not, and as a matter of fact it appears they do. The question then is how to account for the coexistence of two diseases the germs of which are supposed to have an essentially different mode of evolution one from the other. And the matter seems simple. Without analysing all the modes by which the event may occur, it will be sufficient to point out that a soldier or sailor having contracted dysentery is sent into a crowded hospital, where he either finds typhus already established, or creates, or helps to create, it, by contributing fœcal matter which he is unable to remove and which is left. In this way it seems perfectly easy to see how an essentially out-door, and an almost necessarily in-door, disease, may coalesce. For a man sick with one disease will inhale the aërial germs of another disease if they come in his way. So that if a dysentery patient happens to be enveloped in an atmosphere charged with typhus germs, there are no pathological reasons why he should not have typhus superadded to his dysentery. Therefore I conclude that the blending together of typhus and dysentery, in no way militates against the hypothesis of their separate and distinct modes of causation from the same substance.

270. The encroachment of the typhus poison on typhoid patients may of course take place in a precisely similar manner. Given a typhus area and all who come within it will be affected more or less. The overlapping of typhus and typhoid, by the way, may have delayed the work finally done so well by Dr. Jenner, and may yet lead to doubts as to the exact nature of a particular case, or set of cases. All these camp diseases have a facility in engrafting themselves one upon the other and are found combined in many ways—dysentery, however, being the one most constantly present in these combinations. The germs of three or four diseases may invade the same body in paludal regions, or in camps afflicted by a visitation of cholera. Before Sebastopol the surgeons were occasionally uncertain whether the men died from cholera or dysentery. Algidity and cramps were associated with tenesmus and a flux of blood. The certainty with which dysentery will ally itself with all tropical diseases when its germs are introduced into the system has long been noticed. Humboldt alludes to the fact in speaking of the attack of fever his fellow-voyager, Mons. Bonpland, had. Dr. Wibel, however, would appear to consider the thing a singularity and as one to be vouched for. In the letter before quoted he says “I need not point out to you the “interest attached to the simultaneous occurrence of typhoid fever “and dysentery in the same person. I have had myself the “opportunity of witnessing the fact in a man who while on the “point of recovering from typhoid fever, was seized with dysen“tery then epidemic at Bonn, to which complication he succumbed, “the small intestine offering the typhoid, the rectum and large “intestines the dysenteric changes.”

271. In returning to civil life in search of the causation of typhus, the question of water-pollution occurs. How far is this connected with the origin of typhus, or with its propagation? The typhus germs are assumed to be developed originally under conditions the reverse of those required for the development of the dysentery germs, as far as water is concerned. Whereas rain, or a moist state of the air, or a certain amount of moisture in some shape is required for the production of the dysentery germ, the typhus germ seems to be generated under cover and in positions where the only moisture found is that given off by living organisms. And on the mildew hypothesis, I assume that the cryptogam which forms on fœcal matter appears in the shape of a dry mould. With regard to the after-extension of the fungus when it finds its way by means of the discharges, into water, I see no valid reason why it should not continue its existence in the same way, up to a certain point, as the (hypothetical) water-plants of typhoid, cholera, dysentery, &c., nor why it should not then be an effective poison when taken into the system. That poisoning occurs in the rare cases when the typhus germs get into potable waters and these are drunk before the water-plant of typhus has had time to exhaust its substratum, I can easily conceive possible. But that this mode of extension of the disease is extremely rare, is probable for two very sufficient reasons—sufficient at least from the point of view here taken. The first is that the peculiarity in the origin of the typhus germs cuts them off from the external air and from all channels of communication with drinking water—save one. The only ordinary and effective way by which the typhus germs can taint water would seem to be through the discharges of an infected person. The marvellous difference this makes may be seen on comparing the relative chances of water-pollution by typhus and by typhoid germs. Every leaky midden, privy, and cess-pit, and every collection or deposit of fœcal matter in every country, may become accessory to, or instrumental in, the poisoning of reservoirs containing water used for drinking purposes, by typhoid germs. Wherever excrement is exposed on the surface, it may be washed by storm-waters into drinking water; and wherever the ground is sodden with excrement, filtration may take place in endless ways into wells or cisterns; and assuming that the excrement is tainted with typhoid germs, potable waters will be infected. That typhoid water-pollution actually takes place on a large scale in most countries is well known. In fact the Prince of Wales and Lord Chesterfield were both infected by drinking-water, poisoned by filtration from a contaminated source, at Lord Londesborough's. But all these avenues to drinking-water are closed to the typhus germs. They cannot form on exposed excrement in the open air, or on fœcal matter in the soil. The leakage of excrement receptacles in no way adds to the amount of typhus poison evolved in a district, unless when the discharges of typhus patients are accidentally added to the fœcal matter in the saturated ground. Then the excrement may afford pabulum

for the typhus water-plant, and if the fluid in the earth thus infected should happen to gravitate by any means into a reservoir of drinking water, typhus poisoning may take place. But the chances are that at least twenty thousand persons are infected with typhoid germs, to one person infected with typhus germs, by drinking-water.

272. The other reason which appears to me to exclude the typhus germ from being largely causative of the disease by extension from its water-plant, is that the water-plant must soon die out. The hypothesis of the mode of evolution of the typhus germ involves an early extinction of its aquatic form of vegetation. For the conditions of formation of the hypothetical typhus fungus preclude its reconversion from the water-plant to the terrestrial mildew, should circumstances deprive it of the water in which it exists; so that it cannot be a means of infecting air and cannot propagate itself by sporules. In the hypothesis of the typhoid water-plant [173] it is submitted that the oscillations of ground-water play a very important part in determining the amount of typhoid poison in the air of a city, as at Munich, by alternately drowning the mildew and uncovering the aquatic vegetation. The same with the cholera water-plant. But the principle cannot, manifestly, apply to the typhus water-plant, for the reason that, supposing it to be left on the sides of any excavation by the receding water, the surrounding moisture and the air are conditions inimical to the return of the aquatic vegetation to the terrestrial mould or mildew of typhus. As the typhus germ could not be generated under such conditions, it is assumably improbable that the conditions would facilitate the change from the one form to the other. Therefore it seems likely that the water-plant of typhus must always be limited to the particular water-system in which it may be; and that it cannot pollute other waters, either directly or indirectly, through the air, as is assumed to be the case with reference to the water-plants of typhoid and cholera. All things considered, the actual number of persons infected with typhus outside dwellings must be very small indeed. In fact the little attention this possible source of infection would appear to have received, would argue that even if typhus does ever occur in this manner, it is not suspected, or detected. I submit that the hypothesis offered squares perfectly with the rareness of typhus-poisoning by water.

273. The occurrence of typhus under circumstances where neither crowd-poison, nor want, can have been concerned, and where contagion cannot be traced, shows that crowd-poison is not an essential factor of the specific infective agent. The fact would seem to be placed beyond question, for several well-authenticated cases are recorded; and though a thorough contagionist will always have his doubts, the evidence seems conclusive. These exceptional instances of typhus have proved great hindrances to all those who have tried to elicit the causation of the disease. They have always stood right in the way of conclusions and have greatly hampered

or prevented, generalisation. They have been anomalies not to be reconciled with overcrowding by any theory, or to be dovetailed with it by any hypothesis. Some writers have denied them and got over the difficulty that way. Others have dropped them out of consideration altogether. But the mildew hypothesis accommodates itself equally to overcrowding and to no crowding. It takes in both phenomena. The fact that typhus is generated under such opposite conditions is no bar to the reception of this view, which shows how it is that the aërial albuminoid constituents of a crowded dwelling are not factors of the infective agent, and how want is not a necessary condition of its formation. For all that is absolutely required is the presence within a certain confined space of a certain amount of fœcal matter in a certain state. I cannot determine, of course, the conditions as to the quantity of space, amount of fœcal matter, or its state. I could only approximate. But I will not even attempt to do that in my present position of absolute ignorance on these points. I will merely say that I infer, from the imperfect data before me, that dry mouldy excrement left in a place of habitation will infect the air of the place with "low vegetable organisms" capable of producing typhus in those who come within their influence:—and this no matter how clean the place may be in all other respects, or how free the air may be from all other organic impurity. The hygienic arrangements of a house shall be in the most perfect state of efficiency, and yet the introduction of human excrement into any one of its rooms—its dormitories more especially—and the retention of the material there until it becomes dry and mouldy, shall be an efficient cause of typhus among the inmates. It may be of rare occurrence that such a thing happens; but, by the law of averages, it will happen so many times in so many years in so many millions of houses that fœcal matter is voided and left in sleeping apartments. Reference has already been made to the source of danger in water-closets both as regards typhus and typhoid [177]. Typhoid mildew I consider most likely to form on fœcal matter adhering to the valves or pipes of closets in use, and typhus mildew when closets are unused for a time;—moisture being required for the former, dryness for the latter.

274. It has been generally observed that typhus does not occur in the tropics—or that it is extremely rare, if it does occur. Genuine typhus it is said is unknown, though fevers of the same type are a result of overcrowding. "Putrid fevers" for which there is no nomenclature—fevers of a hybrid kind, exhibiting the manifestations of typhoid and typhus in strange combinations—break out on board ship, in unsanitary garrisons, in badly ventilated prisons, and elsewhere. I cannot enter fully, or at length, into this subject, but I suggest that the explanation of these modifications of disease will be found in the different conditions under which excrement is placed in torrid zones. When moisture is present in hot air, cryptogamic vegetation is more rank and rapid and the dry powdery mould of typhus is precluded. A typhoid

mildew may be generated, or another form, or many other forms, of parasite of a kindred type may be developed. When the air of a country, on the other hand, is hot and dry, the dryness may be so great as to exhaust the moisture from organic matter before the germination of fungi can take place. The typhus mould, which requires a certain amount of dryness to perfect it, may be arrested or prevented by complete desiccation of its substratum. With regard to the variations in the symptoms and lesions of typhus in England and elsewhere, they are probably governed by the conditions under which a particular growth of mould occurs and by the special composition of the substratum on which it forms. The degree of virulence of the poison is determined by the accidental circumstances of its formation.

275. The disinfection and deinfection of typhus depend upon the discovery of causation. If that be a mildew, of course the destruction of the mildew and of the sporules and particles given off, is the end of disinfection. Although this is the clear object, however, it will always be practically most difficult to arrive at. But a more difficult problem in hygiene still, is that of deinfection for typhus. One painful conclusion is forced upon the reflective mind in considering this subject; and that is the impossibility of preventing epidemics of the disease in many places by any public hygienic measures, or by any legislative enactments. In England and Ireland, for instance, it seems almost hopeless to expect that the conditions for the spread of typhus can be destroyed, or interfered with to any material extent, so long as destitution and its accompaniments shall prevail. The conditions for the evolution of the typhoid poison may be almost completely obviated by sanitary arrangements. Epidemics of enteric fever may be prevented among the poorest and most densely crowded of populations. But it is not so with typhus. An effective excrement-removal system may be enforced in large towns and typhoid eqidemics may thus be shut out from dense populations. But thorough as this will undoubtedly prove, so far as typhoid is concerned, it cannot do more than a part of the work of deinfection for typhus. For leaving out of consideration for the moment the precise cause of typhus, it is evident that its propagation to an epidemical extent is mainly dependent on overcrowding. And as overcrowding is a direct result of want, it follows that the only hygienic means that can be really successful are those which will relieve want. Whatever may be the nidus in which the typhus germs are hatched originally—whether excrement, or other organic matter—it is clear they flourish in dirty, crowded, dwellings. Therefore, even when the discovery of the source of the typhus germ is finally made, no matter what it may turn out to be, it will not be possible to do more, by corporate acts, than to reduce the chances of the occurrence of typhus in these dirty, crowded, dwellings. It is difficult to see how British subjects are to be coerced into cleanliness, or redeemed from want and its consequences. Typhus epidemics, therefore, it is to be feared, are of

THE THEORY

OF THE

CAUSATION OF DYSENTERY.

[CONCLUDED.]

277. The hope I entertained of being in a position to offer the reader something more solid and tangible than my own views in support of the hypothetical mildew, and, consequently, of the theory of causation of dysentery, has not been realised. Unforeseen obstacles have stepped in to obstruct and delay the proposed microscopical observations; and the germ which I had counted on is not forthcoming. It is unfortunate for many reasons, but it cannot be helped. I must be content to send this forth so much the weaker by the absence of the material stay on which I had half calculated, and on the strength of which I have spoken here and there with more confidence, perhaps, than exact knowledge justifies. It has indeed been urged upon me, by those on whose judgment I should generally be disposed to rely, that I should withhold the publication of these speculations on disease germs until I, can produce some more satisfactory evidence to their soundness. It has been observed, perhaps truly, that there is no real work in them; that they are merely suggestive and prove nothing whatever: whereas if I waited until the parasites which I believe to infest fœcal matter were some of them made out, and the hypothetical mildew of dysentery haply connected with its assumed substratum, there would be completeness and finish in the argument. Practical and scientific men might then be more inclined to look into the subjects mooted; but, as the case stands, they whose opinions have weight in the world are not likely to take up unsubstantiated views and speculative notions, and to examine them with any care or consideration.

278. To this kindly-meant advice I have not deferred—though I cannot but admit the force and justness of the observations so far as they go. But a year or more might elapse before a microscopist, enabled to devote his whole time and give his undivided attention to the subject, could conduct so many sets of experiments as would be necessary for thorough elucidation. And if the microscopist failed after all in bringing out the results I anticipate, I confess I should not take the negative disproof of one man to be conclusive. Nor on the other hand if he succeeded in establishing the correctness of the hypothesis by finding the cryptogam, would the world

at large accept the testimony of one witness. Judging by the reception of Dr. Salisbury's important discovery of the ague-plant, it might be many years before sets of observations made in Australia would be repeated by the *savans* of Europe. The learned might keep the fate of the dysentery-plant [if found in Victoria] hanging suspended indefinitely. That which is now in the womb of time might, if brought forth prematurely and irregularly in this colony, be left in the cold like its elder cryptogamic "American Cousin"—on the *Palmella* side.

279. But a still more important reason weighs with me and impels me to publish at once. If, as I believe, the prevention of dysentery may be compassed by the simple means I suggest, I consider I should not be justified in awaiting the complete verification of hypothetical views which may, or may not be, sound. The nature of the dysentery germ can be determined only by the microscope; and this must in any case be the work of time. But in the meanwhile a large mass of disease may be prevented—by empirical means, no doubt, yet it would be sheer folly to decline a good because its goodness is not clearly understood. If the source of the dysentery germ can be made clear and some rough notions of the conditions under which dysentery occurs can be gathered; and if, in consequence, practical measures which shall have the effect of stamping out the disease entirely can be at once taken, it is of no such great moment to learn precisely what the germ is—so far as dysentery itself is concerned. While therefore the more complex and really more important part of the investigation must be left to skilled labourers, the less learned may very readily test the value of the hygienic expedients suggested. The arguments advanced in support of this part of the subject may possibly attract the notice of some of those in command of large bodies of men placed where dysentery, in the present state of knowledge, is almost certain to be engendered. And perhaps the extremely simple form of sanitation required to ward off this fearful scourge may be tried. If it be done on a large scale hundreds and thousands of lives might possibly be saved while philosophers are discussing how it is done. Therefore I have decided to let this go as it is—"scarce half made up."

280. Although the enforced separation of the subject of dysentery into two parts may give an awkward air to this work, I am not sure but the division may not have had its advantages. It has enabled me to exhibit the relation of the disease to a group of diseases, many of which are so closely connected by their common origin with dysentery that it is impossible to consider the causation of any one of them, without trending on the causation of all the others. And it may have enabled the reader to apprehend more readily the vast extent and ramifications of the enquiry as to dysentery. It may have served to give him a glimpse of the enormous influence that the pernicious material which I take to be the cause of dysentery exercises throughout the world. It may have shown him the priceless value that the true discovery of the

dysentery germ would be; and he may understand my anxiety to have the cryptogam of that disease perfectly demonstrated. For it would have converted what may now, perhaps, be looked on as the creations of a dreamy mind, or as the mere brain fancies of a man possessed with one idea, into solid inductive reasoning upon a sure foundation. It would have given life to what may now be a dead letter. For I do not hesitate to say that if only a little of the mucology of human excrement had been made out; if only one of all these various diseases had been successfully connected with a mildew on fœces;—if the cryptogam of dysentery, for instance, had been found—I do not hesitate to say that the conclusions as to the other diseases *must* have been substantially correct. By all analogy and induction I maintain that if I am granted a mildew on excrement as the efficient cause of dysentery, I prove that a mildew on excrement is the efficient cause of cholera. There is no escape from the logical inference. And I say confidently that, on the same premiss, nothing else can be the efficient cause of typhoid fever. In the present aspect of the question, however, I can only submit that excrement under certain conditions will, in some way or other, cause dysentery; and that under other conditions it is probably concerned, in some way or other, in the causation of the other zymotic diseases. The gap between the ground I now take up and the position on which I should have stood, if I had had the dysentery germ firm within my grasp, is a wide one. Looking into the near future, however, I see that that gap will be closed up by the discovery of the dysentery germ. And on the faith I have that it is a mildew on excrement, I leave my hypotheses as they are; and I now put in my claim, prospectively, to a share in the discovery; and also to shares in the contingent discoveries of the germs of cholera, typhoid fever, typhus, and all the other diseases enumerated. Let who may have the glory of the actual demonstration of these germs, I warn them that if they occupy and cultivate my ground and bring forth fruit, I shall exact my *droits de Seigneur*. I must at least have my peppercorn. But this is idle talk. I shall envy no man his just title to the honour that will be his due, if he shall succeed in evolving the causes of these diseases by real work—a very different thing from spinning hypotheses. I can distinguish between substantial things done and speculative things suggested. The most promising idea is as nothing to a practical result. An acre of performance is worth the whole Land of Promise. And so to the theory of the causation of dysentery—for I have a theory now to handle—not a pure hypothesis. In this particular field, whoever gleans will find little left but the germ and the precise mode in which it produces its specific effects in the human system. The actual source of the germ, and the conditions under which the germ is evolved, and the method of preventing the evolution of the germ, and, therefore, the occurrence of the disease—nearly all that is essential to practical hygienic measures in fact—*is already* garnered. Whether the germ be found or not, the deinfection for dysentery can even now be made nearly perfect. Any nation may rid itself of epidemics of the flux at will, if the germ be never discovered. And if it be discovered, it will make no material alteration, probably, in the sanitary means to be employed—unless with a view to the prevention of other diseases at the same time. If I shall succeed in showing this tangible good, it may be legitimately considered as so much work done—even though done imperfectly.

DYSENTERY IN NEW HOLLAND.

281. I propose in the first place to take dysentery from an Australian point of view. I shall cite instances of the occurrence of the disease in New Holland and state the inferences I draw from them. I shall pass on then to the dysentery of other countries; and test the four propositions submitted on the causation of the flux by the light of medical and historical records. This is the general outline, but it is out of the question to adhere to any precise rules or exact modes. I begin with this country, both because I am more familiar with events here, and because the causation of dysentery is, perhaps, more perfectly and more vividly brought out by the epidemics on the various gold-fields, than by similar outbreaks in other lands in modern times. Taken as a whole, indeed, the facts in connection with dysentery in New Holland are so highly illustrative of its causation, that there is no absolute need to travel elsewhere for proof as to its origin. As these facts, however, may not be generally known, and if simply related by me might be questioned, or doubted, or discredited, I will supplement them by facts derived from sources which shall not admit of doubt or question.

282. Before treating of modern dysentery, it will be as well to go back to the first attack of the malady in this country of which there is any account. And this was almost simultaneous with our taking possession. In White's Journal of a Voyage to New South Wales, the writer, who was Surgeon-General to the Forces, enters "on the 29th of Jan. 1788:—In the course of the last week, all the "marines their wives and children, together with all the convicts, "male and female, were landed. The laboratory and sick tents "were erected, and, I am sorry to say, were soon filled with "patients afflicted with the true camp dysentery and the scurvy. "More pitiable objects were perhaps never seen. Not a comfort "or convenience could be got for them, besides the very few we "had with us. His excellency, seeing the state these poor objects "were in, ordered a piece of ground to be inclosed, for the purpose "of raising vegetables for them."

283. What was the cause of this outbreak of dysentery so soon after landing on the shores of Port Jackson? Was the disease endemic from malaria? Or was it brought thither by contagion? Or was it spontaneously generated by the new comers? The

question of malaria on Australian soil has already been dealt with [259, &c.] It could not have been the cause. As regards contagion, it appears that the transport ships conveying this, the first batch of convicts, were attacked with dysentery shortly after leaving the Cape. They sailed on the 13th of November, 1787, and on the 17th White says:—"The wind variable, inclining to "the southward and eastward, with hazy weather, an epidemic "dysentery appeared among the convicts, which very soon made "its way among the marines, and prevailed with violence and "obstinacy until about Christmas, when it was got under by an "unremitting attention to cleanliness, and every other method "proper and essential for the removal and prevention of contagion. It gives me pleasure to be able to add, that we only lost "one person by this disease, violent and dangerous as it was; and "that was Daniel Cresswell, one of the troops intended for the "garrison; who was seized on the 19th of November and died the "30th of the same month, the eleventh day of his illness. From "the commencement of his disorder, he was in the most acute "agonising pain I ever was witness to; nor was it in the power of "medicine to procure him the shortest interval of ease. His case "being a very singular one, I have transmitted it, with some "others to a medical friend," &c. The next entry on this subject is made 20th December—"I visited the Prince of Wales, where I "found some of the female convicts with evident symptoms of the "scurvy, brought on by the damp and cold weather we had lately "experienced. * * * On those days the scurvy began "to show itself in the Charlotte, mostly among those who had the "dysentery to a violent degree; but I was pretty well able to keep "it under, by a liberal use of the essence of malt," &c.

284. The Surgeon General makes no further reference to dysentery, either before the fleet arrived, or after the convicts were landed. Assuming that the attack of dysentery had been "got under" about Christmas, there would have been an interval of a month or thereabouts before the patients began filling the sick tents. It was not until the people—1030 souls all told—had been on shore for awhile that the disease broke out again. This at least is the conclusion I draw from the somewhat obscure and imperfect narrative. Assuming this, there is nothing conclusive upon the point as to the introduction of the flux into the country. Contagionists might consider the outbreak on land a revival of the epidemic at sea, and might account for it on the supposition that lingering seeds of the disease yet remain on disembarkation, and germinated rapidly under more favourable conditions. Certainly the account admits of this way of explaining the fact of the recurrence of dysentery. For there can be no doubt that the expression "got under" is vague, and that dysentery is not a complaint to be stamped out in six weeks. Such an epidemic as that referred to would probably leave a few chronic cases to be imported into the settlement. Therefore those who hold to the

view that contagion is concerned in the causation of this disease, will be able to trace the operation of this cause here.

285. For my part, looking at this sudden occurrence of dysentery at Sydney by reflection from the more recent endemic attacks elsewhere in New Holland, I conclude it to have been a spontaneous generation of the flux. And, moreover, the previous attack on shipboard was clearly a result of the creation of the germs *de novo*. For the convict fleet lay four weeks at the Cape; during which time no mention is made of dysentery, either by White, or Collins, or Hunter, or any of the narrators of the event which happened on the voyage. Besides it will be noted that "epidemic dysentery appeared among the *convicts*, which very soon "made its way among the marines." So that even supposing the malady was prevailing at the Cape while the transports were there and that the historians omitted to chronicle it, the convicts were the last persons on board to have contracted it by contagion. The sailors and marines might have gone on shore but the felons would have been confined to the ships. The only assumption left to contagionists by which to account for the commencement of dysentery among the convicts, is that the affection was raging at the Cape unknown to the visitors, that some water-casks were filled with "bad water" and some with untainted water, and that the former was supplied to the criminals. There is no other way of explaining this epidemic among the convicts by communication with an infected source; and it is not a satisfactory one. It involves too many improbable conjunctures.

286. The interpretation I put on the outbreak of dysentery in the settlement on the shores of Port Jackson, is far more simple than the explication that it was a resuscitation of an epidemic which occurred on board ship from contagion, at the Cape. Over a thousand people were landed in the bush without latrinal arrangements or closet accommodation of any kind. The natural result followed—an accumulation of fœcal matter on the surface of the soil within a short distance of the encampment. The convicts of course would be kept from straying far; and the hostility of the natives prevented the others from separating themselves from the main body: so that the whole party, cooped up for the first month within a small area, was surrounded by excreta deposited only so far off as decency demanded. Here there were all the conditions for the development of the dysentery germ—save one. Moisture at the end of January in N. S. Wales is not common. At that period the atmosphere is generally dry and the country parched. Unfortunately for the newly arrived settlement, an exceptional fall of rain came in less than a week and supplied the only condition wanting for the production of the germ. White says:—"Feb. 1st.—We had the most tremendous thunder and "lightning, with heavy rain, I ever remember to have seen."

On the next day—"This morning five sheep, belonging to the "lieutenant-governor and quarter-master were killed by lightning "under a tree." No more need be said. In that climate, with these conditions, this attack of dysentery was a consequence.

287. Following up the history of the penal settlement for many years, there are no more accounts of epidemic dysentery on so large a scale and with such fatal results as this first outbreak. For although the Surgeon General does not mention the mortality, Governor Collins says, speaking of the landing:—"The tents for "the sick were placed on the West side [of the Cove]; and it was "observed, with concern, that their numbers were fast increasing. "The scurvy, that had not appeared during the passage, now "broke out; which, aided by a dysentery, began to fill the hospital, "and several died." [I may observe also that Lieut.-Col. Collins corroborates White's account of the rain. "The weather during the latter end of January and the month of February was very close, with rain, at times very heavy, and attended with much thunder and lightning."—p. 20.] But after this foretaste of epidemic dysentery on Australian soil there were no further serious attacks. By degrees the settlement shaped itself more in accordance with English notions. The provisional arrangements that were compulsory at first, were soon discarded for rough contrivances by which the evolution of the dysentery germ to any large extent was precluded. As the successive shiploads of felonry arrived at Port Jackson the convicts were generally attacked on landing; on which Col. Collins (August, 1791) observes—"It "might have been supposed, that on changing from the wholesome "air of a ship's between-decks to the purer air of the country, "the weak would have gathered strength; but it had been "observed, that in general, soon after landing, the convicts were "affected with dysenteric complaints, perhaps caused by the "change of water, many dying, and others who had strength to "overcome the disease, recovering from it but slowly." It was not the water however. These fresh batches were landed under somewhat similar conditions to the first arrivals. It was some little time of course before they could be quartered and placed on the same footing with the others. They could not all be absorbed into comparatively civilised life at once, but had to pass through the same phase of existence with all settlers in the bush. Hence surface pollution and its attendant dysentery. Nothing more is heard however of dysentery epidemics. For although dysentery has figured largely on the bills of mortality of N. S. Wales ever since the colony was founded, a wide-spread propagation of the disease has been impossible. Endemic attacks have occurred yearly, at particular seasons, at those spots where a small amount of surface defilement of the ground has taken place; and they occur there still, as they do in this colony and all the Australian colonies: but it was not until the discovery of gold threw men together by thousands in the heart of the bush, that the original *epidemic* was repeated.

288. Flinders landed some men to die of dysentery in Sydney in the year 1803; and, as his account of the disease is interesting and instructive, I insert it here. When cruising in Torres Straits in the Investigator, the ship's company became affected with scurvy. At page 247 of his Voyage Flinders says, "I was myself disabled by scorbutic sores;" and the survey on which he was engaged had to be given up. South-west winds having driven him towards Timor he "judged it advisable to obtain refreshments there." He dropped anchor in Coepang Bay on 31st March 1803. The Investigator sailed again on the 8th of April. During her stay part of the ship's company were allowed on shore. Captain Flinders and some of his "prinicipal officers and gentlemen" dined with the Dutch Governor at his residence. "To judge," says Flinders, "from the appearance of those who had resided any length of "time at Coepang, the climate is not good; for even in comparison "with us, who had suffered considerably, they were sickly looking "people. Yet they did not themselves consider the colony un-"healthy, probably from making their comparison with Batavia; "but they spoke of Diely, the Portugese settlement, as very bad in "this respect. Captain Baudin had lost twelve men from dysentery, "during his stay at Coepang." On the 21st of April there is the following entry:—"Dull weather, with frequent heavy rain, thun-"der and lightning, had prevailed from the time of leaving "Coepang, and it produced the same effect upon the ship's com-"pany as similar weather had done before in the Gulf of Car-"pentaria; for we had at this time ten men in the sick list with "diarrhœa, and many others were slightly affected. It seemed "possible that the change of food, from salt provisions to the "fresh meat, fruit and vegetables of Timor,—a change by which "I hoped to banish every appearance of scurvy, might have had "an influence in producing the disease; and if so, it was avoiding "Scylla to fall upon Charybdis, and was truly unfortunate." In May I find—"From the 27th of April we steered eight days to the "S.S.W." "The diarrhœa on board was gaining ground, not-"withstanding all the attention paid to keeping the ship dry and "well aired, and the people clean and as comfortable as possible. "Some of the officers began to feel its attack." On the 17th of May the boatswain died. On the 18th W. Hillier, "one of my "best men, also died of dysentery and fever before quitting "the bay, and the surgeon had fourteen others in his list, unable "to do any duty. At his well-judged suggestion," * * * "more room was made for the messing and sleeping places; and "almost every morning they were washed with boiling water, "aired with stoves and sprinkled with vinegar, for the surgeon "considered the dysentery on board to be approaching that state "when it becomes contagious." On the 21st of May:—"The "sickly state of my people from dysentery and fever, as also of "myself, did not admit of doing anything to cause delay in our "arrival at Port Jackson." May 23—"This day James Green-"halgh, sergeant of marines, died of the dysentery." May 30—

"When I contemplated eighteen of my men below, several of "whom were stretched in their hammocks, almost without hope, "and reflected that the lives of the rest depended upon our speedy "arrival in port," &c. "On the 2nd of June we lost John Draper, "quarter master, one of the most orderly men in the ship; and "it seemed to be a fatality, that the dysentery should fall "heaviest on the most valuable part of the crew." June 8—"Whilst beating against this foul wind, the dysentery carried off "another seaman, and had the wind continued long in the same "quarter, many others must have followed." On the 9th of June they were landed at Sydney; "but four were too much exhausted, "and died in a few days."

289. The allusion to the Gulf of Carpentaria refers to some illnesses, not apparently of a very serious character, for the following is the only entry, made in the preceding February, with regard to them:—"Several of our people were ill of diarrhœas at this "time, accompanied with some fever, which was attributed by the "surgeon to the heat and the moist state of the atmosphere; for "since December, when the N.W. Monsoon began, not many days "had passed without rain, and thunder squalls were frequent. "Exposing the head uncovered to the sun, more especially if "engaged in strong exercise, was proved to be very dangerous "here; I lost one man in Blue-Mud Bay from a want of due precaution in this particular, and at this place two others very "narrowly escaped." But there were no deaths from the diarrhœas and fever; and the probability is, therefore, that when the Investigator got to Timor on the 31st of March, there had not been a case of dysentery amongst her crew up to that time.

290. This account of ship dysentery contracted at an infected port is a good typical account of ship dysentery. It is bristling all over with points as to causation; but the principal one to be noticed now, is the contrast between the coast of Australia and any of the inhabited islands off the North coast and on the other side of Torres Straits. The Investigator had been running along the Australian coast from Port Jackson for months, during which time the crew had gone on shore frequently for water and had met with large numbers of natives here and there; and yet there is not a sign of malaria at any part of New Holland. No sooner, however, is a Malay population met with than the most deadly malarias of all kinds are sure to prevail. [I have not quoted the account Flinders gives of fever in Timor, but it is worth reading.] And the Malays found active coadjutors in the fabrication of specific poisons in the Portuguese of those days. That people have been great planters of colonies in every part of the world; and wherever they have taken root, dysentery, "putrid" fevers, remittents of the most fatal type, yellow fever, cholera, and every zymotic disease that could possibly gain a footing in the latitude in which they were, have gathered round them. It matters not where you go—east or west, in either hemisphere, in the torrid zone and in semi-tropical regions—it's always the same story.

The Portuguese settlements are sure to have dysentery, at any rate, permanently attached; and generally some one or two other epidemical febrile diseases hanging about as well. The Spaniards also have proved nearly as certain extensive breeders of infection. These two nations were notoriously the most regardless of any European nation up to a recent period, of the mode in which they disposed of their excreta. The Dutch also, strange to say, were much the same before and at the beginning of this century. They were far more cleanly than the English in the interior of their houses, as Sir W. Temple found when he spat on the floor—"having a great cold"—whilst dining at Mons. Hoeft's, and "a tight handsome wench [that stood in the room with a clean "cloth in her hand] was presently down to wipe it up, and rub "the board clean." But while the Hollander has been scrupulously neat and abhorrent of dirt inside his dwelling, he has not been so mindful of outside pollution, or of public decency, as the Briton. That reform in national customs which ended in the general population of England resorting to the use of receptacles for their excreta, preceded a similar reform in Holland by nearly a century. Hence the state of Coepang, Batavia, and the other Dutch settlements in the East, at the beginning of this century. The British possessions were malarious also, it is true; but where the Englishman got a permanent footing and settled himself down to live, he surrounded himself with the means of observing privacy as regards natural functions and also took especial care to restrict the excreta of his household to certain defined places—matters about which the other nations did not then appear to be over solicitous. The result has been that a purely English quarter in a tropical city has always been less malarious than a purely Dutch quarter. At least it was so. Now the Hollander would seem to have gone a long way ahead of us in practical hygiene, if one may contrast the sanitary measures taken at Batavia with those adopted at Calcutta. To be sure the Dutchman has rather a summary way of settling native questions. He does not allow himself to be poisoned out of a scrupulous regard for the feelings of the race he has subdued. What he thinks ought to be done, he does, with directness and singleness of purpose. He has no Exeter Hall.

291. The voyages of the Beagle from 1837 to 1843, as given in Captain Stokes's Discoveries in Australia, were some of them over the same tracks as those of the Investigator in 1803. There are some interesting facts, illustrative of malaria, in Captain Stokes's narrative, to which I will briefly allude. The Beagle anchored in Gage's Road, Swan River, Nov. 15, 1837. Stokes says:—"The "25th Nov. was fixed for our departure, when most unfortunately "Captain Wickham, while on his way to Perth, was attacked with "a severe dysentery, and continued so ill that he could not be "brought to the ship till the end of December." There is very little more said of this attack, except that Captain Wickham could not shake it off. The sloop in course of time went to Timor, and

the following entry occurs:—"On every hill was presented the "contrast of redundant natural verdure, clothing its sides and "summit, and of cultivated fields along the lower slopes. These "by irrigation are turned into paddy plantations, the winds blowing over which give rise to those insidious fevers, intermittent, I "am told, in their character, which are so prevalent at Coepang, "as well as dysentery, from which indeed the crew of the Beagle "afterwards suffered." They were in Coepang in July. The next allusion to the matter is:—"From the very debilitated state of "some of the crew, from dysenteric affections contracted at Timor, "we were not able to leave Swan River before the 25th of October."

292. In 1841 Captain Wickham retired from the command of the Beagle and Captain Stokes was appointed to her. Captain Wickham "had never thoroughly recovered from the attack of "dysentery he experienced on our first arrival at Swan River." The foot-note at p. 362, Vol. II., is so important that I must extract it in full. Captain Stokes says—"The following remarks "from Mr. Bynoe, on the climate of Northern Australia, corroborate the views put forward in the text:—"

'I find on a reference to the Medical Journals, as well as to a 'Meteorological Table kept by me during a period of sixty years, 'on the coasts of Australia, that we had no diseases peculiar to 'that continent, and I am led to believe it a remarkably healthy 'country. On the North and North-West coasts, where you find 'every bight and indentation of land fringed with mangroves, 'bordering mud flats, and ledges formed by corallines in every 'stage of decomposition, with a high temperature, no fevers or 'dysenteries were engendered.

'Our ship's company were constantly exposed, in boats, to all 'the vicissitudes from wet to dry weather, sleeping in mangrove 'creeks for many months in succession, pestered by musquitoes 'during the hours of repose, yet they still remained very healthy; 'and the only instance where the climate was at all prejudicial (if 'such a term can be applied) was in the Victoria river, on the 'north coast, where the heat was, at one period, very great, and 'the unavoidable exposure caused two of the crew to be attacked 'with *Coup de Soleil.*

'Our casualties consisted of two deaths during our stay on the 'Australian coast, one from old age; and the other, a case of 'dysentery contracted at Coepang.'

'It may not be uninteresting to state, that from the time that 'Port Essington was settled in 1838, up to the period of our last 'visit to that military post, and for some time after, no endemial 'form of disease had manifested itself, and the only complaints 'that the men had been suffering from were diseases such as were 'usually to be met with in a more temperate clime, and those were 'few. But we must take into consideration their isolated position, 'the constant sameness of their life, their small low thatched cottages, mostly with earthen floors; their inferior diet, and also 'the absence or scantiness of vegetables. Most of the men, more-'over, experience a constant yearning for home, which, yearly 'increasing, terminates in despondency, and leaves them open to 'attacks of disease. Scorbutic symptoms were at one period very 'prevalent, arising principally from the poor form of diet; similar 'cases occurred in a former settlement on that part of the coast, 'from the same causes; but although Port Essington has been of 'late visited by sickness, I do not consider it by any means an 'unhealthy spot.'

293. There is one more reference made to disease by Capt. Stokes and a significant one. The Beagle visited Timor again and she sailed from Coepang on the 24th of September, 1841. On the 26th "the first lieutenant, the surgeon, and the master, were seized "with a violent attack of cholera, which lasted 24 hours—another "evidence of the unhealthiness of Timor."

294. The reader will note that the only instances of infectious disease during the voyage of the Beagle occurred at the Swan River settlement, where Capt. Wickham was attacked with dysentery, and at Coepang, where the crew also contracted that disease, and where the slight touch of cholera was experienced. As with the Investigator so with the Beagle; neither of these vessels found the slightest indication of any malarious disorder on any part of the Australian coast frequented by the indigenous races only. Capt. Wickham fell among Europeans. It is unnecessary to go further into the accounts of the numerous voyagers who have explored these seas. One and all bear testimony to the salubrity of the Terra Australis; and to the insalubrity of the Malayan Islands. If now we turn from the vast extent of the coast of New Holland to the interior of the country, the same remarkable immunity from zymotic diseases will be found. There is not a single instance of disease of this stamp in any one part of the whole continent among the aboriginal races; while in the instances in which exploring parties have been visited with disease of any kind, the visitation has been unconnected with indigenous malaria—except the ague miasm. Even in Capt. Sturt's expedition into Central Australasia when all the officers of the party were affected with scurvy, and Mr. Poole died of it, there is no mention of dysentery or fever. The dysentery spoken of in the disastrous exploration of Burke and Wills was probably a misnomer. It was a bowel complaint accompanied with tormina, and perhaps tenesmus, brought on by eating "nardoo"—the fruit of an acrid cryptogamic plant—*Marsilea quadrifida.* So also the dysenteric affection experienced by Governor Eyre on two occasions during his memorable journey along the coast. The diarrhœa both he and his faithful black boy Wylie had, came on after killing a horse and partaking of the dried flesh too freely. They recovered at once as soon as their systems had got rid of the noxious material. The dysentery alluded to in Davis's Tracks of McKinlay across Australia, p. 207, was probably true dysentery—the only case of the kind I have met

with in Australian exploration. As I believe it to be unique, I have endeavoured to trace it out to its causation; and to my mind the explanation is simple. The entries occur as follow:

"14th." [Jan. 1862] "Bell taken very ill with cramp in the "stomach. We thought he was going to die right off. He was "quite doubled up and could not speak. I gave him some medi-"cine which restored him in a short time. At one time we thought "it was really all up with him."—"15th. Bell and I very ill from "dysentery. The heat did not contribute to our recovery. The "sun comes through these American drill tents [I was about to "say '*like*'] *without* winking."—"16th. We are still very ill, and "yesterday another of the party, Maitland the cook, was taken "with the same disease. He suffered very much at first. It must "be the weather, or the water, or perhaps both combined. Mr. "McKinlay and party returned about 1 p.m. and found us on our "beam ends. The sooner we are out of this nasty hot and sickly "camp the better."

[Mr. McKinlay's own journal contains the following:—"16th. "Arrived at camp at 2 p.m. Found some of the party, viz., Bell, "Davis, and Maitland, laid up with dysentery, the former seriously. "Have made up my mind to leave this, after one day's spell for "the camels, and go back to different water, as this must contain "some medicinal properties that I am ignorant of, and affects all "of us more or less—no doubt the weather has a good deal to do "with it; the heat is fearful."]

Davis continues. "17th. Intensely hot and oppressive. Not "very good for the sick fellows. I (Davis) am better, but the others "are not. It is the general opinion that there must be something "in the water that makes us all so unwell."—"18th. We are off "from this infernal sickly hole."—"19th. We bathed to-day. "Bell still ill. Bathing will soon set us all right."—"22nd. The "sick slowly improving."—"23rd. This is about the nicest camp "we ever had, and the bathing, don't mention it. Bell is better, "but he has had a stiff time of it." Nothing more is said about Bell's dysentery. The party camped by this good water-hole and McKinlay set them to dig wells [for employment]. On the 31st I find the following—referring to a pool of putrid water fallen in with away from the camp:—"What is it that turns the water "bad? Is it the accumulated dung of the wild fowl, and the ex-"cessive heat of the sun, or what is it? This I leave to more "scientific men than myself" [258].

"Feb. 1st. The fellows still sick after their meals; Ned very "bad, also Maitland, but I have escaped the infliction."—"8th. "Raining splendidly."—"9th. It is still raining."—(10th. Party left camp) "15th. Mr. McKinlay taken violently ill with dysen-"tery"—"Mr. McKinlay very ill this afternoon; he must have a "very serious attack, as his face is very much pinched since this "morning, and he walks really very ill indeed."—"16th. Remained "in camp. Mr. McKinlay in his tent all day, and looks worse than "ever."—"17th. Mr. McKinlay still very ill."—"19th. Middleton "was taken so ill by the way that we were unable to get him to camp "that night."—"22nd. McKinlay quite well to-day and Middleton "improving." And during the remainder of the journey there is no more dysentery.

295. Assuming the malady by which five out of this party of seven were attacked in the interior of New Holland to have been true dysentery—and McKinlay was too old and experienced a colonist to have confounded diarrhœa with dysentery—several most interesting matters present themselves for consideration. In the first place the little party started from Adelaide in the middle of August 1861, and before the end of September they left all traces of the white man. From the commencement of the journey until the 14th of January 1862, there is not a word about dysentery in Davis's Journal. For three months and a half they had not seen a hut occupied by an European. The breaking out of dysentery at Lake Blanche, where they were encamped at the time, was therefore as pure an instance of an endemic disease as could well be conceived. Contagion is completely shut out—from European sources. It remains then to consider from what other causes it could have sprung. The chances against contamination from the natives are enormous. For setting aside the fact that the aboriginals in the neighbourhood had shown no signs of the disease, it is marvellously unlikely that the explorers should have fallen in with a tribe differing from all other tribes that had ever been met with in any other part of Australia. Then as regards water-pollution, or water containing material capable of causing dysentery, it would be singularly strange if they happened to camp on the only two water-holes that have dysenteric qualities—the only two samples of "bad water"—in the whole of New Holland.

296. It appears to me that the explorers, while camped for several days, brought this attack of dysentery on themselves by some error connected with the disposal of their excreta. At one camp the weather was hot and oppressive—which in Australia means that the air contains moisture—with thunder and lightning; and at the next camp they had abundance of rain. Supposing therefore the individuals of the party went only a short distance from the camp to void their excreta, there were all the conditions for generating the dysentery germ and for the infection being carried by a favouring current of air to the tents. As these attacks of dysentery are the only attacks I find recorded in exploration annals, I am somewhat inclined to the opinion that the camp regulations as to excretions were not carefully attended to, or not clearly laid down, or not understood, or not thought of. The party, I suspect, did not reflect sufficiently upon the difference between a camping place for one night and a camp where they were to halt for several days. The men at the depôt that Sturt left when he endeavoured to penetrate into the centre of the continent did not contract dysentery. Nor did the encampment under Brahé, left at Cooper's Creek by Burke and Wills, experience a touch of the disease all the months they remained there.

297. In fine after an attentive perusal of most of the works of the explorers, I have been unable to find any similar case to that of the attack of McKinlay's party; or any reference to zymotic disease among the natives. Although scurvied and famished frequently, they all escape diseases unconnected with nutrition. Oxley, Grey, Mitchell, Leichhardt, Strzelecki, Stuart, Gregory, the Kennedys, Austin, Howitt, Walker, Landsborough, and others, may be searched in vain for any allusion to dysentery among the blacks, or among their own parties.

298. By way of confirming the views of Mr. Hodgkinson, Mr. Bynoe, Capt. Stokes, and those who have written of the singular absence of malaria in New Holland, I may refer the reader to an interesting account of the salubrity of Somerset in Jardine's Journal. Somerset is a settlement at the extremity of Yorke's Peninsula, between the 10th and 11th parallels of latitude. In the Appendix to the Journal will be found a letter, dated 1st March, 1865, from the Police Magistrate, Mr. Jardine, (father of the writers of the Journal) to the Governor of Queensland, Sir George Bowen, giving the details of the settlement; and another letter dated May 22, 1865, to the Governor from T. J. Haran, Surgeon, R.N., reporting on the climate. I cannot afford space to quote largely; Mr. Jardine, however, says:—"Of the agreeableness and salubrity of the climate of Somerset, I cannot speak too favourably"—"as far as can be judged there is no *local malady*. There "has been no symptom of fever or ague." Dr. Haran says, *inter alia*, "The dreaded summer season, with its calms, light winds, and "heavy rains, has passed off without causing a single case of "sickness, attributable to noxious exhalations, which prevail at "that season in most tropical climates."—"One well-marked case "of scurvy became developed at the end of January."—"Since then "the entire adult community have enjoyed very good health." And I maintain that every country in the world would be as free from noxious malaria [always excepting intermittents] as the tropical parts of Australia, but for men—and possibly monkeys.

299. Water-pollution with dysentery germs, or rather the presence in water by natural means of some substance which creates dysentery, is referred to as a probability by both McKinlay and Davis. This is not an uncommon belief among bushmen, and indeed among others; but it is a fallacy which a little careful and rigid examination will suffice to explode. The question of impure water as a cause of dysentery in New South Wales, however, has been laid down the other way by some authorities. Thus the Rev. Dr. Lang, in his History of that country, says:—"The three forms "of disease that are most frequent in the colony are *ophthalmia*, "*dysentery* and *influenza* or *catarrh*."—"Dysentery is also confined "chiefly, though by no means exclusively, to the lower classes of "the labouring population; and mercury, in doses that a medical "practitioner in Great Britain would be afraid to administer, is "the grand specific whenever it occurs. It is occasioned some-"times by drinking water containing a solution of alum; at others

"by drinking cold water in hot weather, when the body is in a "state of perspiration; it arises occasionally from injudicious exposure to the sun in summer; but its most frequent source is "dissipation." [Vol. II., p. 30.] Perhaps the most valuable part of this extract is that relating to the frequency of dysentery among the labouring population. The views of the Reverend Doctor as to causation need not be considered.

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300. I must now take the reader back again to the early days of New South Wales—so far back as to the commencement of the colony in 1788. The newly-founded penal settlement at Port Jackson quickly threw out an offshoot—a scion of felonry. In the middle of February—less than three weeks after they landed—the Governor [Phillip] sent Lieutenant King with a small party (23 in all) to Norfolk Island in the Supply. They landed safely and the Supply returned and was sent back with convicts. She made several voyages backwards and forwards, always carrying relays of convicts; and when the Sirius was wrecked on a sunken reef close to the island and the crew were landed, there were 506 souls there—on half rations; for the Sirius had been sent with provisions for the relief of the famished settlement. And yet when Lieutenant King left in 1790 he wrote as follows:—"As a proof of "the salubrity and wholesomeness of the air, it is to be remarked, "that there had been scarcely any sickness since I landed, nor had "we any illness whatever, except a few colds." And Governor Hunter, the year after, says:—"Norfolk Island is also subject to "sudden changes, but is also remarkably healthy. * * * And "there has but one old woman, who was sickly before she came to "the country, and one infant, died of a natural disease on the "island, since it has been settled."

301. Whence this immunity of Norfolk Island from dysentery? How was it that men landing on the shores of Port Jackson got the disease and the same class of men on arriving at the Island did not get it? The conditions of heat and moisture were rather more favourable to the development of dysentery in Norfolk Island than at Sydney. There was a rich, deep, alluvial soil, too, in place of the hard, argillaceous crust, or layer of sand, just covered with an apology for mould. And yet with all these aids dysentery was not engendered. This singular and seemingly somewhat erratic departure from the ordinary laws which appear to govern the development of the germ, mystified me for a time. I was convinced that the question of the disposal of excreta was somehow bound up with the explanation, but I could not arrive at it at first. Narratives for general reading do not supply information on such a subject. On searching carefully, however, through Lieut. King's account of the place, I fell upon a little code of regulations put in force in the Island, containing the following practical bit of hygiene. Clause V. "The women are to sweep round the houses "or tents every morning, and to cook the victuals for the men;

" and every person is strictly forbid cleaning any fish or fowls in " or near the houses, but to *go to the sea-side* for that purpose." The italics are mine. The sea-side suggested the train of thought which led to the elucidation of the absence of dysentery. I soon saw that the Island owed its perfect freedom from all those pests which civilisation holds in its train, simply to its physical conditions; and to the fact that for a long while the human beings who had effected a lodgment had been unable to penetrate into the interior—by reason of the supple-jack and the tangled mass of vegetation creeping round and about the pines. The settlement had in fact to clear a space for standing room even. Hence perforce it hugged the coast; and hence the ocean was naturally converted into the receptacle of all depurated organic material, which it received for the most part before there was time for decomposition. All the refuse excrementitious matters of the little penal colony were committed to the deep at once; and the tremendous surf that swept round the island bore all trace of them away forthwith. Thus the accidental circumstances that surrounded the settlement saved it from the horrors of dysentery.

302. That this is the explanation will I think be evident when the subsequent history of Norfolk Island is taken into account. The English Government decreed that the settlement should be abandoned and orders were sent out to the Governor to that effect. So attached had the expirees become to the place, which they had partially cleared and cultivated, that they would not leave. This was in 1805. The Secretary of State, however, was peremptory: instructions came that everybody was to be taken off, by force if necessary, and the Governor was authorised to shoot those who resisted! The island was thus denuded of population in 1808. Some years afterwards it was again occupied as a penal settlement for the worst class of offenders. All the most dangerous ruffians transported from Great Britain were soon deported from N. S. Wales to Norfolk Island. A much larger establishment had to be provided for them than on the first occasion of utilising the place as a convict depôt, both on account of the larger number of felons, and because of their character. A considerable military force was sent down to keep them in subjection. The published records of the island will supply the reader with information as to the horrors of this Inferno, but they will not furnish him with the ready means of determining the causation of the diarrhœa, dysentery and fever to which the troops and convicts were occasionally subject. I have endeavoured to elicit some particulars from officers who were stationed on the island, both military and civil, as to the excrement-disposal system in vogue; and though the information I have gathered is not very precise, it is sufficient to establish clearly the intervention of fœcal matter.

303. The convict establishment being separate and distinct from that of the military, a different system of excrement-disposal obtained: and indeed there always must be a difference in this respect between gaols and barracks everywhere. The excreta of

those prisoners who were detained in the establishments close by the beach were daily cast into the sea. There were two of these establishments, one on each side of the island; and both of these were so placed as to allow of the excreta receptacles being emptied into the ocean without delay. But there was an intermediate station, or depôt, called Longridge, to which what were called the "new hands"—i.e., convicts who had recently arrived and who had not been on the island previously—were sent. There were generally some three hundred of these at Longridge and their excreta were not consigned to the sea, but were utilised as manure.

304. The military barracks were furnished with the usual latrinal arrangements; viz., deep cess-pits which were emptied by the prisoners as occasion required, the contents being partly thrown out of the island and partly consigned to the soldiers' gardens. It may be added that the number of prisoners in Norfolk Island of late years was 2100, and that there were 200 soldiers. Besides these there were the civil servants. Soldiers' wives were there too, and their children, as well as the wives and families of the officers and civil servants. Altogether there were generally from 2300 to 2500 souls on the island.

305. Here then were many of the conditions for diarrhœa, dysentery and fever. During the few years that my informant was on Norfolk Island—from 1840 to 1844—not one death occurred among the soldiers or their wives or children. They were, however, subject to periodical attacks of diarrhœa—attributed to the abundance of fruit. The prisoners had, in addition to diarrhœa, both dysentery and fever—more particularly at Longridge. There were some deaths, but I could gain nothing definite as to numbers. Probably the Parliamentary Blue Books would give the statistics, but they are not at hand and the point is not material. One death from dysentery, or fever, is sufficient to illustrate my position. I submit that the only sufficient explanation of the fact that whereas no malarious disease appeared in the first little settlement, but did appear in the second larger settlement, lies in excrement-pollution. There could not be a more convincing proof of the creation of malaria by man; or a more perfect exemplification of the part which man's excreta play in the transformation of pure air into impure air. It is, in fact, a demonstration of the proposition that civilisation has hitherto begotten its plagues and pestilences by neglecting to cast out its own ordure.

305. The subject of dysentery in Norfolk Island leads to the dysentery met with here and there among the natives of the different groups of Islands in the Southern Pacific. Hearing that it occurs in Fiji, Samoa, Tongo, and throughout Polynesia, both among the native races and those Europeans who have settled down there to grow cotton, I made enquiries. A cotton-planter who had visited many of the islands, gave me some curious information as regards excrement-disposal. It would appear that although the customs of these savages vary somewhat in details in the several islands, there is one custom common to all of them

when living on the sea-coast. The habits of those living inland, in the mountainous parts of the larger islands, was not known to my informant. They who inhabit the country adjoining the sea and where a shelving beach admits, invariably walk out at ebb tide, or a little distance into the water, to perform the offices of nature; and even where the shore is rocky, they still void their excreta into the sea, or perhaps into streams running directly into it. In one island rather thickly populated where there is a sort of hut-city on the edge of the ocean, a law obtains that the citizens shall defecate only after dusk, and in the sea. In other islands they observe matutinal customs, or have no fixed periods.

306. In some of these islands dysentery never occurs; in others it is not uncommon, and there the native doctors have some skill in treating it with certain acid fruits and astringent herbs. In fact my informant, who had himself been subject for a long time to a chronic dysentery contracted on his own plantation, went so far as to say that a remedy compounded for him by a native physician had done him vastly more good "than all the doctor's stuff he had swallowed." But the point now is how some of the islands escape dysentery and others are subject to it. I found that where the beach runs out shallow for some distance, inside the reefs, which frequently act as a breakwater, the excreta are sometimes prevented from getting out into the open sea and are driven towards the mouths of creeks, or into little bays, where they collect and are perhaps drifted up on shore and left. Therefore it happens that at certain periods there are large accumulations of fœcal matter along the beach; and thus the pestilential exhalations of dysentery are accounted for. It is on those islands more fortunately placed, where there is no hindrance to a speedy riddance of the material, that I suppose dysentery does not appear.

307. The foregoing refers to those Polynesian Islands untenanted by Europeans as yet—unless perhaps by a missionary. On the islands where plantations have sprung up, a more artificial state of affairs has been brought about. To say nothing of the town of Levuka, where the disease was rife at one time, but is no longer serious since the people have got privies, the conditions attending life away from the coast are such as to encourage the dysentery germ. The planter who gave me the particulars I have mentioned does not live in Fiji. I have little doubt, however, but that the description of his own plantation will serve for most of the others. The land he has under cultivation is about half a mile or so from the sea-shore and the huts of his labourers are handy to their work. The result is that these men [Polynesians] do not go regularly down to the beach to pass their evacuations, as they would under ordinary circumstances. They follow the habits of Hindoos, or Europeans in the bush. The consequence is surface-pollution and a good deal of dysentery—though, for some unexplained reason dysentery of a less malignant, or acute, kind, than that of Australia. It appears with Europeans to pass rapidly into the chronic stage and to be rarely fatal, unless when, after con-

tinued exposure to the vitiated air, the victim dies from exhaustion. The islanders themselves, whether from their remedies, or from some physical peculiarity of conformation, do not seem to suffer to the same extent as the Europeans who contract the disease. With regard to the milder form which the flux assumes in Polynesia than elsewhere, the only explanation that occurs to me is that the nature of the food of the South Sea Islanders may have some effect in modifying the constituents of their excreta; and by this means lessen in some way the virulent action of the poison germs. The climate and the slight exertion the islanders have to undergo to procure the means of subsistence, demand very little nitrogenous material. They live almost entirely on yams, plantains, cocoa-nuts, and fruits of various sorts—with fish occasionally and, more rarely, human flesh. Even on the cotton plantations they do no hard work, as we understand hard work, and therefore they do not alter their diet to any appreciable extent. I learn that the excreta of the Polynesian have neither the form, nor the consistence, of those of the European; and that they approach more nearly in appearance to cow-dung than to human fœces. The alvine evacuations are more voluminous and contain a far larger proportion of water than those of the more omnivorous feeder. This variation from the usual type of fœces may beget a corresponding alteration in the poisonous qualities of the substance.

307. That the Polynesian dysentery may, however, take a more formidable shape, under other conditions, will be seen by the following slip from the *Daily Telegraph* of Melbourne in November or December 1872. The extract is otherwise too vague for any present purpose :—

"The Polynesian Traffic.—All communication having been
" temporarily suspended between the schooner Jason and the
" shore, we have been unable to obtain any particulars of her
" voyage. Dysentery has been raging on board to a fearful
" extent, and we believe that eighteen of the Polynesians she
" brings have died from this cause. No medical report has yet
" been received by the agents, but as the Jason is expected up at
" the wharf to-day, we shall, no doubt, learn all about the sad
" fatality that seems to have attended her present trip.—*Mary-*
" *borough Chronicle.*"

308. There is very little more to be said about these South Sea Islands. Of course typhoid fever is met with here and there as the sure accompaniment of dysentery. It is not improbable that as the land is more and more cleared, and more cotton is grown and a larger number of hands are employed, the greater amount of surface contamination may lead to water-pollution—even if it has not done so already. I regret that I have been unable to procure any data from the French colony in New Caledonia. There I should expect to find evidence to connect dysentery with fœcal matter on the surface of the soil. I suspect that some of the small military expeditions into the interior have been attended with the

flux—as they are almost invariably elsewhere in the world. However the meagre information one has is sufficient to establish a fair *primâ facie* case that wherever dysentery is found amongst the islands in the Southern Ocean, there exposed excrement is also to be found; and further, that where there is an exemption from dysentery, human excreta are not present under the conditions [submitted to be] essential to the causation of the disease.

THE DYSENTERY OF THE GOLD FIELDS.

309. I come now to the Victorian Gold Fields; and I premise that the soil which was the site of the workings was perfectly free from all suspicion of pollution in any shape or form; and the air exempt from every phase of malaria. There was not the slightest human taint and not the faintest breath of vegetable miasm, where Ballarat, Castlemaine, Sandhurst, Beechworth, or any of the large mining towns, now stand, when the first rush to the ground took place in 1851 and 2. The country was lightly timbered, or open grass land. The soil had not been previously turned, or occupied, save by a shepherd or two. There were no large encampments of aboriginal natives at any of the spots; and if there had been their occupation would have been quite innocuous. Their habits in the matter of their excreta are absolutely prohibitive of dysentery. The practice they have of deinfecting their excrement at their regular camping grounds, is as effective, though not so useful, as that of the Japanese. And inasmuch as the subject will have to be referred to, in order to show how the aboriginals of this country have never been known to have dysentery, typhoid fever, or any zymotic disease, [small-pox excepted] it may as well come now, so as to exhibit clearly the purity of the country and its freedom from epidemics before the white population took possession and introduced civilisation.

310. The difficulty I had in solving the problem of the exclusion of these diseases from the native tribes has already been mentioned. [257.] After satisfying myself that it was a fact, by going through the narratives of the explorers and by closely questioning such old bushmen as I met, I made enquiries with a view to bring out whatever peculiarities there might be in their system of excrement-disposal. I had no success for a long time. Every squatter I spoke to insisted there was nothing unusual in their habits in this particular. The blacks followed precisely the same practice as the whites when travelling through the bush. There could be no mistake, because they had been out for weeks and months with black fellows and had observed them in the act over and over again. It then occurred to me to ask what was the largest number of blacks they had ever seen collected in any one spot. This question was variously answered—from two to between four and five hundred. Had they had opportunities for observing these camps often, or for any length of time? Frequently and for weeks together. And they were morally certain

that at these camps the aboriginals adopted the bush custom in vogue among travellers? Most certain. Then they had observed the results, or detected the disgusting effects in the neighbourhood of the camps? But here one and all were at once nonplussed. Well—no—they really couldn't say they ever had noticed anything of the sort. Yet if they had been frequently at and round these large gatherings of the natives, they would surely have met at some time or other with unmistakable traces of the offensive material? No doubt—but still they could not call to recollection that they had. Indeed when they came to reflect, they were all sorely puzzled to reconcile the fact which they had observed with the negative result that they were bound to admit. They saw clearly enough the irreconcileable nature of their statements. If the aboriginal left his excreta on the surface of the ground, of course it was impossible that such an extensive superficial defilement as would accrue from three or four hundred aboriginals could have escaped observation. And yet they could have no doubt, either as to what they had seen, or as to what they had not perceived.

311. Convinced that there was some simple way out of this difficulty, I lost no chance of finding it. I could not get beyond the point already arrived at, however, for some time. At last I met with a young settler but an experienced bushman—one of those adventurous spirits who have pushed into the far interior and taken up country along the line of Burke and Wills. His run is on the Bulloo, not far from Cooper's Creek, and he employs the young Bulloo blacks as station-hands. Mr. S—— had frequently been at Cooper's Creek and knew the tribe there—to which the Bulloo blacks belong. I put the usual questions to him and got the almost stereotyped replies. He could not at first account for the absence of all noisomeness in the vicinity of the larger and more permanent camps; but after a minute or two of perturbed thought, he suddenly laughed out "Oh! I recollect now—I know how it is." He then told me that having ridden up one day with one of his black boys to a camp, he was leaning on his saddle talking to one of the tribe, when he observed a black fellow go a little distance from the party with a "yam-stick" in his hand, with which he excavated a small hole in the ground. As this was an unusual proceeding—for the gins generally do the delving—S. watched without appearing to do so. To his surprise the native had no sooner finished with the yam-stick than he defecated; and after having fulfilled this natural office, he covered his excreta carefully and effectually with the earth he had removed—effecting this by one or two adroit turns of the foot. The thing struck S. as being so strange, and so unlike anything he had heard, read or seen, that when he rode off he questioned the young native touching this peculiar custom. From him he learnt that this was invariably the rule "'long a' camp," or when the tribe was assembled at any of the more permanent encampments. Whenever they remained, or intended to remain, at any spot, in force, they always adopted this precautionary measure; but when they

were out in the open bush, away from their habitations, they did not give themselves any trouble about their excreta, but left them on the surface.

312. Here then was—to me—a full, complete, and conclusive explanation of the fact that these wandering tribes of Australian savages had never been assailed by the zymotic plagues of the world, while the civilised races had engendered them in the very same country under precisely analogous conditions. For what essential divergence is there between the physical conditions of 500 men living under canvas, and 500 men sheltered by bark gunyahs or miamis? Fortified by the results of numerous instances that occur to me, I lay it down as a canon that a little "rush" of 500 miners to any locality in New Holland (high mountain ranges excepted) must, under the past and present mode of surface soilure, be presently attacked with endemic disease—if only rain comes or heavy dews fall. On that sole contingency of moisture would depend whether the party should be overwhelmed with dysentery or typhoid fever, or should not have to expiate unsanitary sins of omission: whereas a tribe of native blacks would be secure in any weather, by virtue of their wholesome attention to associated hygiene, or to their instinctive dislike to malodorous emanations and to uncleanness. For it is not quite clear whether the blacks have fallen into their camp usage from mere dislike to foulness, or whether they have adopted it in consequence of any experience of the disastrous effects of leaving the deleterious substance uncovered in their vicinity. It is difficult to divine which of these two motives has actuated the Australian savages in keeping their camps free from fœcal abomination: but, judging from the fact that they are not over-cleanly in some other directions, and taking into account the disregard shown, or the toleration displayed, by the Hindoos, Chinese, Malays, Papuans, and most black and many white races, in the matter of stercoraceous effluvium, I should be rather disposed to think that at some time or other these blacks have had some severe lessons in connection with this subject;—lessons which may have opened their eyes to the dangerous nature of the material and led to their simple and efficacious way of deinfecting it. There is nothing improbable in the supposition that some of the old men in the tribes observed a relation between disease and the excreta of camps; and came to the conclusion that, as sickness always came on when a fœtid air was wafted in amongst them, the sickness was the result of the fœtor.

313. The miners of this colony started with a pure atmosphere and an unpolluted soil. They settled down for the most on ground situate among low quartzose, or schistose, hills, with shallow valleys, or open flats, between. Those who broke up the surface to pass the earth through cradles, or sunk their rude and shallow shafts, rarely met with more than an inch or two of true alluvial soil, except perhaps in the actual beds of old water-courses. The tents of the diggers, storekeepers, sly-grog sellers, and of the

heterogeneous mass of people who go to make up a mining population, were pitched generally a little distance from the actual scene of operations. They were thrown together in clusters of eight or ten, or fifty or a hundred, as the nature of the ground, or some accidental circumstance, might determine—at first without order or arrangement, but subsequently with some attempt at regularity and with some view to the general convenience. A certain space round each tent was provided for by the regulations and thoroughfares were settled. But it was a long time before the confusion attendant on such a state of affairs was overcome. The especial point in connection with this peculiar phase of society which relates to the present question, is the mode in which eight or ten thousand people jumbled up together in this way dealt with their excreta. They who have been present at one of these precipitate gatherings will readily recall the surroundings; but readers who have not been at a "rush" might fail to realise what took place, unless their attention were drawn to the consideration of what must of necessity have taken place. Each mining centre was for the time being an Indian village, as regards excrement-disposal. There were no latrinal or receptacular arrangements of any kind among the miners. In fact no one thought of cleanliness or infection, for all were intent on getting the gold out of the ground as quickly as possible. The quantity of fœcal matter, therefore, that accumulated during the dry months in some of the environs of the larger fields, [and generally within a short distance of the tents—for few men would go far away for many reasons] must be computed by scores of tons. The general results of this condition of affairs has already been given in Par. 132, 133, 142 and elsewhere. I have, however, a few more particulars to set down.

314. When the moisture came and every gold-field was suddenly stricken with a plague of dysentery and typhoid fever, (and all the fields nearly simultaneously) there was one locality where the tents were jammed closely together in places, and where about three or four thousand people had collected within a more limited area than usual, owing to the configuration of the country. Here some small flats were enclosed and intersected by low-topped hills with level crests. The somewhat narrow ridges of these hills had been utilised as cloacal ground to an unusual extent. When the dysentery epidemic broke out, the mortality among the residents in this neighbourhood was most startling and the rapidity with which those attacked died almost unprecedented. Few lasted more than nine or ten days and the majority sank in three days; while in some few instances death took place in less than 24 hours from the commencement of the flux. The few medical men who were in the neighbourhood of Fryers Creek were astounded by the terrible virulence of the epidemic as it appeared about Murderers Flat. Had it not fortunately happened that the yield of gold temporarily fell off at the spot, or that other attractions elsewhere quickly drew away the miners, the victims would soon have been reckoned by hundreds, instead of by scores, weekly.

315. There was one class of men, on most of the larger fields, who did not contribute to the sum of surface-pollution. The officials very soon segregated from the mass of human beings and lived altogether apart from the tumultuous crowd. Before the epidemic broke out, the Commissioners of Gold Fields, the Police Magistrates, the Police Officers, together with the staff of clerks, the whole Police Force, and all the tent-keepers and others in the service of the Government, were established in Camps, where they existed in a more decent fashion. These Camps were generally placed, when practicable, on a hill, and a large area of ground around them was proclaimed as a Government Reserve. At the rear there was sufficient privy or latrinal accommodation for all the persons living within the precincts of the Camp. There was no occasion, therefore, for their resorting to the bush outside the limits of the Reserve. When dysentery fell upon the miners, storekeepers and others, the Camps were all spared by the epidemic. A few policemen and those whose duties took them some distance outside the Camp boundaries at night were attacked; but the Officers and those who were not called upon to go off the Reserve after sundown escaped altogether, or one or two perhaps had very slight touches of the malady. At all events not one official died from dysentery in 1852-3 at the time when the disease was raging. The wife of one Commissioner died at what was called an Out-Station. This was a small Camp where only one Commissioner and his clerk and a few police were stationed. The Reserves on which these Out-Station Government Camps were planted were far smaller than the Reserves for the head-quarters of the districts. In this instance, where the Commissioner's wife died, the Camp Reserve was small and was moreover in close contiguity to the tents of the general population. It was therefore directly subject to the same malarias.

316. It should not be omitted from this account that when claims were deserted by the miners and when open shafts were left in the ground, or any kind of excavation that could be made available for the purpose, the diggers who worked near at hand converted these places into excreta receptacles. So that fœcal material in considerable quantities was thus left exposed in the very midst of working miners.

317. Reference has been made on several occasions to the fact that epidemic dysentery has not occurred in the high ranges of Gipps Land, or in the neighbourhood of Woods' Point. This divergence from the uniform appearance of the disease, under the required conditions, in all other parts of the colony where new gold workings have been commenced, soon forced itself on my notice and engaged my attention. I have investigated the mode of excrement-disposal at these diggings as well as time and means would allow. There are certain variations in the distribution of the material arising out of the physical peculiarities of these mountainous localities, but the change is not to so great an extent in all cases as to be apparently an essential difference, or a difference

likely to lead, of itself, to results different from those found elsewhere. The amount of surface-pollution in the ranges and gullies at an altitude of from 3,000, to 5,000, feet, has been considerable here and there and the mining communities in the immediate neighbourhood would seem to have been as much exposed to malaria there as elsewhere. Typhoid fever has been developed at Woods' Point and was extremely prevalent and fatal at one time, though it is no longer formidable; but dysentery does not appear to have been engendered anywhere in these mountainous districts. It was rife in the Beechworth District in the early days at a height of between 1,500 and 2,000, or 2,500, feet above the sea level. It may be mentioned with reference to typhoid fever, that at Woods' Point many of the miners used a deserted tunnel running into the side of a hill as a latrine, and that it soon became converted into a most pestilent hole. Several diggers in the neighbourhood of this tunnel died from enteric fever. A correspondent who has informed me of this, states that he once had occasion to pass near the mouth of the tunnel and the stench from it was so horribly nauseating that nothing would induce him to go into the vicinity again.

318. After the first year or two the miners generally brought their wives and families to the diggings. The storekeepers and others did the same. This involved greater attention to the decencies and amenities of British communities. Arrangements had to be made in consequence of the introduction of this element into the population; and gradually the change was effected by which dysentery epidemics were precluded. Ballarat, Castlemaine, Sandhurst, Beechworth, and some of the mining townships that have sprung up since, are now as cleanly and as fairly scavenged as English towns of the same size. The result is that dysentery and typhoid epidemics are no longer possible. Isolated cases of these diseases have been reduced in number yearly; and these towns now are on a footing with Melbourne as regards mortality. Dysentery is constantly heard of on the outskirts of these more settled communities; and must necessarily continue to be generated until the masses are indoctrinated with the danger of leaving their excreta exposed. When men shall thoroughly apprehend the poisonous effects of the material they now think so lightly of, dysentery, typhoid fever, cholera, plague, and many other diseases will—as I believe—be all swept away. As regards dysentery the thing is certain. The inference from the accumulated facts relating to this colony alone is too convincing to be resisted.

319. Since the older Gold Fields broke out, "rushes" innumerable have occurred in every part of Victoria; and I do not know of an authenticated instance in which dysentery has not been an attendant—when the local and seasonal conditions have been present. And its constant associate, typhoid, or, as it is more commonly known here, "colonial," fever, is there also. Indeed when the conditions do not favour the development of the dysentery germ on surface excreta, typhoid fever never fails. So that every "rush" in Victoria has ended in poisoning to death a certain

proportion of miners and others, not one of whom could have died from dysentery, if all of them had covered their excrements with soil; and probably not one would have been attacked by typhoid fever—though on this point I speak with less confidence. Even down to the present day, although the surroundings of a rush now are very different to what they were formerly, the dysentery germs are almost always developed. There have been two little rushlets here lately—one in the neighbourhood of the Pyrenees, of which I can learn nothing definite as yet, and the other in Gipps Land. Of this latter field I learn that both at Turton's and Stockyard Creek a few cases of the flux have occurred, though fortunately they have not assumed a malignant form. Diarrhœa has prevailed also and one or two cases have been very severe. There was an epidemic on a small scale of influenza. [I may remark in passing that a very severe epidemic of this disease has lately been prevalent on the sea-board of Western Australia.] Typhoid has not made its appearance at Stockyard Creek, as I am informed by a correspondent—though, as there is no medical man at either of the places, cases may have occurred without their becoming known.

320. These small collections of miners now-a-days do not end in the rapid and extensive surface pollution which used to occur formerly, even in rushes just as small, or smaller. The sum of poison germs is therefore lessened by so much; and there are neither so many, nor so malignant, cases in proportion to numbers as there used to be. But let one of the old-fashioned rushes occur, such as those of Korong, Maryborough, Avoca, and many others, of from ten to twenty thousand men, and a dysentery epidemic is a certainty, if rain, or moisture in the air, should be coincident with, or should soon follow upon the rush. This has been the history of all rushes, not only in this colony of Victoria, but in New South Wales and Queensland. And history repeats itself.

THE DYSENTERY OF MELBOURNE.

321. The epidemical attacks of dysentery to which Melbourne was subject in 1853, 4, and 5, as well as the sporadic occurrence of the disease in this city and suburbs up to the present times, have already been referred to. The very remarkable stages of social development through which Melbourne passed during the first four or five years after the discovery of gold in Victoria, must be specially pointed out however. Every account, or history, of the colony will supply the reader with the statistical details of immigration and of the rise and fall of Canvas Town. It is sufficient for my purpose to state that when the gold-fields began to attract the people in October and November 1851, Melbourne was a city of less than 24,000 inhabitants. The first effect of the gold discoveries was of course to lessen this number considerably. But most of the earliest adventurers soon returned to Melbourne with *their gold* and the neighbouring colonies were rapidly affected with

the tidings. They poured all their superfluous manhood into this city as fast as the coasting steamers and vessels could bring the throng that pressed hitherwards. Melbourne began to fill to overflowing in less than a year. With the aid of deal quartering a number of temporary buildings were run up and house accommodation of some sort was provided for a while. But towards the end of 1852, the arrivals from the outside world commenced and followed one upon the other, so as not to permit of their being absorbed without great difficulty. Shipload after shipload came swarming in and Melbourne could not expand fast enough. Every available corner of every habitation being preoccupied, there was nothing for it but canvas for the new-comers. Almost all the vacant allotments of land in the back parts of the city and in the low lying lands about Collingwood and Richmond were squatted upon. These, however, were presently found insufficient for the wants of the ever continuous stream that was pouring in. More space was required and Canvas Town was started. By the beginning of 1853 it numbered some thousands of people. Some idea may be formed of the rate of the increase of Melbourne by the census returns. On the 2nd of March 1851 the population (city only) was 23,143. On the 26th April 1854 the city and suburbs held 81,904 souls; and of these more than one half, or over 40,000, arrived between November 1852 and the taking of the census.

322. The special feature to be drawn attention to in this peculiar posture of affairs is the excrement-disposal system—or rather absence of system. If the reader will take thought, he may picture to himself what must have been the state of a place capable of affording, by energetic management, and supposing the sanitary arrangements to be in efficient working order, privy or closet accommodation for perhaps 30,000 people, when called upon to provide for the wants of double that number at a time when social matters were in a state of disorganisation. All the ordinary means and appliances of city depuration were thrown out of gear. Every night-man who owned a horse and cart had left for the diggings and scavengers were not to be had. As for sinking cess-pits, or making privies, labour was too high and wood or other material much too valuable for such purposes. The result was of course that every midden, privy, cess-pit, and receptacle was soon full to overflowing; and their contents welled over into back yards innumerable. But this was not all. The occupiers of the tents on the allotments in Melbourne had no kind of privy accommodation whatsoever. The excreta of this section of the people, therefore, were thrown, or deposited, on the surface. The thousands of residents in Canvas Town—that medley collection of tents, shanties, stores and grog-shops covering the rising ground on both sides of the St. Kilda road and spreading towards Emerald Hill and St. Kilda—were in a similar predicament. This exceptional community was in a singularly unfortunate and awkward position—a worse position by far than miners on a new gold-field; for there was no bush and the tents were in full view of the city.

The result was inevitable. Although the Government of the day made some attempts, for decency's sake, to provide latrinal accommodation, these public places of resort were not available, for many reasons; and the people were compelled to improvise private expedients. The consequence was that Melbourne and its suburbs were soon in a most indescribably filthy condition from fœcal pollution—both on the surface and in the soil; as may be readily conceived, when it is remembered that all hygienic machinery was at a stand-still and the excreta of from 60,000 to 80,000 people were unremoved.

323. From the end of 1851 to about the beginning of 1854, very little could be done effectively in the way of scavengering, or sewage removal. All that time the municipal authorities were paralysed, by the extent of the pollution and by the impossibility of procuring adequate funds and labour. About the end of this time affairs began to assume a more settled shape, for the truly herculean task of cleansing had been commenced. The tents of Canvas Town had been struck, by Proclamation, either at the end of 1853, in or the first few months of 1854—I forget the exact period—and although a large number of people still lived under canvas, the greater proportion were housed. Tents were more sparsely dotted about West and North Melbourne, Collingwood and Richmond. Wages were reduced, though still enormously high, and the Corporation of Melbourne at last found men to do the sanitary work of the city. Such, however, was the accumulation that for a long time the hands employed could make but little effect upon it; for the daily wants of the community were so great that it was hard work to keep pace with them. Privy convenience was even yet most insufficient; and, consequently, surreptitious surface-pollution went on to a large extent. Besides all these drawbacks, the civic world was divided from the commencement upon the question of the mode of cleansing the city. The surface drainage party was pitted against the underground sewage party; and while these two parties were debating on the merits of their respective proposals, temporary expedients had to be adopted. A good deal was done by the people themselves. The open allotments were enclosed and the squatters in tents were warned off. The vagrant and homeless were restricted to certain quarters by degrees; and as the gold-fields became more easy of access, the new-comers were not detained in the capital so long. So that although the actual quantity of excrement left in the midst of the population was in proportion to numbers, yet it was no longer spread over such a large extent of ground. It was more concentrated at particular spots.

324. When Melbourne began to attract the notice of the adjoining colonies about 1837, or 1838, and a small population lived there, chiefly in tents, dysentery appeared in the autumns of two or three years following the settlement; and old residents speak of the Melbourne of those days as having its sickly seasons. Heat and moisture combined generally produced dysentery, diarrhœa

and "colonial" fever; but when permanent buildings were erected and the place assumed the proportions of a town, it became more healthy. There were always a few localities to which these diseases clung, but in the year 1851, when gold was discovered, Melbourne was in a fairly healthy condition. The sanitation was not equal to the requirements, but it was on a par with English towns of the period and the mortality was less. In the early part of the autumn of 1853, the year following the epidemics on the gold-fields, the city and suburbs were visited with a plague of dysentery, diarrhœa and fever, nearly as disastrous as the separate plagues that had ravaged the different diggings. What the mortality of the population was during the year 1853 can only be guessed at, but it must have been something fearful. Unfortunately the vital statistics of the colony could not be taken, for the Registrar General's Department shared in the general disorganisation; and neither on the gold-fields for several years, nor in Melbourne from 1851 to 1854, are there any records of a precise nature as to deaths and the causes of deaths. Computing the mortality of 1853, by that of 1854, and judging by the accounts given by the medical men who were in Melbourne that year, the number of deaths from dysentery, diarrhœa and fever must have been something startling. Canvas Town and the low-lying part of Flinders Street near the wharves and facing Canvas Town, suffered most severely, but the malaria was diffused over almost all parts of the city.

325. In order that the reader may see at a glance the position of Melbourne as regards dysentery and diarrhœa from 1854 to the end of 1871, I append a Return obligingly furnished to me by our able statistician, Mr. Archer, the Registrar-general. I regret that I did not request him to add the mortality from fevers, but I did not apprehend the intimate connection between them and dysentery at that early period of my enquiry*.

326. I cannot stop to analyse this Return carefully and minutely. I must leave the reader to draw his own conclusions. He may readily contrast the mortality as compared with the population in 1854 and 1871. I may add to these statistics that in Westgarth's Victoria [1857] I find (at page 40) the returns of burials in the New Cemetery gives 3307 burials there in 1854. There were probably a few more in the old burial-ground and in the suburbs. Of these 3307 deaths I observe that 1066 occurred between the ages of 20 and 50, 1034 under a twelvemonth, and 642 above one year and under five years. The monthly return shows that in January there were 517 burials, in February 397, and in March 393: or 1307 in the first three months of the year 1854. In April they fell to 274; and in July to 174. In December they had risen to 275. Taking the indications to be derived from these statistics, together with the fact that the sanitation of Melbourne was begun in the same year which admitted of registration, I

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* This table will be found in the next page.

RETURN showing the number of DEATHS from DYSENTERY and DIARRHŒA, which have occurred in MELBOURNE AND SUBURBS during the years 1854 to 1871 inclusive.

	Number of Deaths from—			POPULATION,
	Dysentery.	Diarrhœa.	Total.	1851 [2nd March] 23,143 city only.
1854 (year end. 30 June)	743	206	949	1854 [26th April] 81,904 city and suburbs
1855... " ...	527	222	749	
1856... " ...	255	120	375	
1857... " ...	225	150	375	1857 [29 March] 128,490 city and suburbs
1858... " ...	301	276	577	
1859... " ...	275	261	536	
1859 (6 mo. end 31 Dec.)	107	107	214	
1860 (year end. 31 Dec.)	275	261	536	
1861... " ...	150	226	376	1861 [7th April] 139,916 city and suburbs
1862... " ...	143	351	494	
1863... " ...	110	249	359	
1864... " ...	94	287	381	
1865... " ...	124	448	572	
1866... " ...	118	481	599	
1867... " ...	117	446	563	
1868.. " ...	63	324	387	
1869.. " ...	74	363	437	
1870... " ...	72	313	385	
1871... " ...	78	273	356	1871 [2nd April] 206,780 city and suburbs

Registrar-General's Office,
Melbourne, 28th October, 1872.

W. H. ARCHER,
Registrar-General.

assume that the mortality of the city in 1853 must have been greatly in excess of that of 1854, even though the population was considerably less. The epidemics no doubt followed the same course in Melbourne as on the gold fields and were less severe on their reappearance the second year.

327. The population of Melbourne and suburbs on the 26th of April 1854 being 81,904, it may be assumed that the mean number of people in the city in 1853 was 60,000, or thereabouts. The deaths from dysentery in 1854 were 743, and if it be assumed that they were, in round numbers, 1000 in 1853—though I believe this to be very far short of the actual number—the reader will observe that this estimate represents a very formidable epidemic. It is difficult to compute the proportion of fatal cases among those affected, but the malady was observed to be less virulent, or malignant, in Melbourne than on the diggings. It was extremely violent in a few instances in certain localities—[Dr. Y. informed me that a man, one of his patients in Flinders-street, died within the day after the beginning of the attack]—but on the whole the

epidemic was decidedly milder in character than the epidemics on some of the gold-fields. The explanation of this will be suggested presently; but now the point is the proportionate number of deaths among the infected. If it be taken at one in five—though one in ten is a high rate—it would give an epidemic of dysentery extending to 5,000 in a population of 60,000—or a twelfth of the people attacked with one disease during one year. If to dysentery be added fever and diarrhœa, the sickness and mortality from these three diseases must have been appalling.

328. During the period that Melbourne was visited with epidemic dysentery, the gaol in the heart of the city and the stockade at Pentridge were not attacked. They contributed no more than their usual share to the death-rate. In the case of the Gaol this was very trifling, but at the Stockade there was a larger mortality from dysentery. For some years the number of deaths from this disease in Pentridge was relatively greater perhaps than among the free population. But when the prison was completed, dysentery was extinguished. While the building of this immense pile with its vast solid wall of blue-stone was going on—a work which was done to a large extent by the prisoners themselves—the gangs were mostly employed at the quarries and sometimes at portions of the ground away from the central part where the privies were. This arrangement entailed surface-pollution to some extent—even though the ordinary portable water-closets were in close proximity. In the old and unworked quarry-holes the prisoners would sometimes defecate; and though the sum of fœcal matter deposited in this way within the area of the Prison Reserve may not have been large in the aggregate, yet it was sufficient, in a climate like this, to lead to the formation and dissemination of a sufficient number of germs to infect a certain proportion of those who came within the sphere of their distribution. Some of the prisoners were attacked and two or three died yearly, so long as that state of things lasted. No sooner was the place completed and outside labour on the wall, and especially about the quarries, ceased, than dysentery ceased also. From that time to this the Pentridge Prison has been exempt from dysentery; and, it may be added, from gaol fevers—with two exceptions. Some years ago the prisoners in the Panopticon complained of foul air in their cells; and one of them was attacked with fever and died. At the inquest Dr. Reed, the Resident Surgeon, stated that in his opinion the fever, which was of a typhoid type, had been caused by vitiated air, from some imperfection connected with the drainage from the cells of that part of the prison. The inquest was adjourned and a great deal of evidence was taken, the general tendency of which was to show that Dr. Reed's views were illusory and that drainage had nothing to do with the occurrence of the malady within the Panopticon. The enquiry, however, ended by the drain pipes being subsequently examined, when they were found out of repair and blocked up so that an accumulation of fœcal matter had taken

that, whether the theory which has been propounded by me be finally accepted or rejected, at all events the theory that dysentery is caused by the generation of a specific poison from the decomposition of organic matter *in* the soil will have to be abandoned. This single case of Melbourne, even if it stood alone, would suffice to controvert the proposition of Professor Maclean. And here let it be understood that while I am compelled to dissent from the views of the distinguished Professor, I would do so in all courtesy. If I have finally singled his opinions out from amongst those of other English authors, for the purpose of procuring opportunities for deploying my own arguments, it has been principally because he has, in my opinion, gone nearer to the true explanation of dysentery than other English writers. In fact he has only just missed it. If it be considered presumptuous in an unknown and obscure theorist to have ventured on criticising and opposing the views of eminent men, I can only say that in the realms of induction there are for me no Gesler's caps. If I were an astronomer I should not, necessarily, accept the calculations of a Newton or a Herschel.

334. As the decrease of dysentery in Melbourne was in an inverse ratio to the increase of excrement pollution in the soil; and as I conclude therefrom that the decomposition of organic matter in the soil has no relation to the causation of dysentery; I submit that the only inference left is that the cause of the disease in this city was due to the presence of fœcal matter on the surface of the soil. It was remarked a little ago that the epidemic of 1853, in Melbourne, was not so malignant as the first epidemics on some of the gold-fields in 1852. It may be added that it was not so wide-spread, proportionately to the population. There were sufficient reasons for this. In the first place the mining population was confined almost entirely to men, during the first year. These adult males, being in the bush, felt no such restraints as to the mode in which they obeyed the calls of nature, as they would have felt in Melbourne or its suburbs. They merely went away from the tents to a distance regulated by individual feelings or convenience. So the excreta of the miners were left exposed on the surface, in the most favourable condition for the development of the largest amount of dysentery germs [Prop. 3.] That is to say, each deposition of the substance, by being generally separate and apart from other depositions in the vicinity, offered a larger superficies to the air in proportion to bulk than obtains under most other modes of excrement-disposal. Hence when the required moisture came, there was the widest possible field for it to operate upon; and hence the production of the germs was on a correspondingly large scale. It will also be borne in mind that the digging communities lived at that period entirely under canvas and that they were all compelled to resort to the bush—except the trifling numbers living on the Government Camps.

335. The population of Melbourne were in a very different position. Not more, certainly, than one third of the people, if so

many, were in tent—which makes a vast difference as regards the propagation of infection by malaria: for it may readily be conceived that no tent can be such a protection against the intrusion of mist-borne germs as a house. Then again the relative quantity of fœcal matter left on the surface, was less in Melbourne. But what contributed most, perhaps, to reduce the amount of dysentery germs given off was the manner in which the excreta of those who lived in tents, and had consequently no privy accommodation, were disposed of. The denizens of Canvas Town and of the tents on the open spaces in Melbourne—the two market-places by the way were covered by the tents of license-holders from the Corporation—were unable, as may be supposed, to adopt the custom of the diggers. They were a mixed population too of men, women and children. Many of them were well-bred; and the shifts and contrivances to which they were compelled to resort, must have been painful and humiliating in the extreme. The only point pertinent to this enquiry, however, is the general result of this state of affairs. The result was that while the men mostly retired to certain spots, of limited area, after night-fall, or before sun-rise, the women and children were obliged to use temporary receptacles which were only emptied late at night or at early morn, at some place tacitly understood by the neighbourhood: and although this unwritten law of the community was not regarded by the filthy, it was faithfully obeyed by a large section of the inhabitants. The consequence was that a considerable proportion of the excreta of Canvas Town found its way into the swamp between the present Military Barracks and Emerald Hill; that the mode of deposition in masses did not favour the production of the dysentery germ; and that the constant daily additions to the deposits disturbed the formation of the germ. It appears to me that it was from all these causes combined, that Melbourne suffered far less in the dysentery epidemic of 1853, than the gold-fields in their simultaneous epidemics of 1852. Probably also the half-scurvied condition of the miners rendered them less able to bear up against the flux when they were attacked; and thus contributed to the greater mortality.

336. It is much to be regretted that the present system of registration and the present effective condition of the Registrar-General's department, did not obtain in the first years of the gold-fields. It would have been most interesting and instructive to have exhibited the early bills of mortality from dysentery at the principal diggings, side by side with those of Melbourne. As the matter stands, I could only speak loosely and roughly as to the subject; my observations, therefore, would lack that support which a background of precise and authoritative information only can give. As they at a distance, who have no means of verifying statements, would perhaps hesitate to accept conclusions founded upon facts which might appear to them doubtful or apochryphal, I will reserve those conclusions for other similar facts. The history of dysentery affords numerous parallel instances to those of

portionately to the other classes of the population. If then we deduct from the mortality of Melbourne and suburbs all the deaths from dysentery that may properly be considered as extraneous, and for which the sanitary arrangements of the City and Suburban Municipalities are not justly responsible; and if we take into calculation the other conditions referred to as conducive to the formation of the dysentery germ; it will be found that the sum of surface-pollution—as represented by its resultant—is after all very trifling; probably not greater than that of any English city and suburbs of the same dimensions and population at the present time. Dysentery can only be accepted as a measure of surface-pollution in countries having similar climatic conditions. The fact, therefore, that so many deaths occur in Melbourne yearly from the flux, is not to be measured by an English standard, but by the dysentery rates of that portion of Europe in the same relative position to the equator. The latitude of Melbourne being 38, let the reader run his eye along the parallel on the map of Europe, take the cities a little on either side of it, and compare the mortality from dysentery in them, computed on the basis of population, with that of this city. This would be a fair test as to the degree of external cleanliness of the respective cities. It will be found, if I am not much mistaken, that the sum of excrement-contamination of the surface of any one of these cities—as expressed in dysentery—is from 8 to 12 on the Continent to 1 in Melbourne. If we then compare the total of fœcal pollution, both above and below the surface, of Melbourne and suburbs for the last five years—as represented by its outcome in dysentery and diarrhœa, typhus, typhoid and relapsing fevers, and diphtheria; [for I cannot, as yet, consent to exclude this disease from the list of those caused by fœcal matter]—if we compare this total with the total of that of any city in England, we shall find that Melbourne is in a better hygienic position than any one of them, as regards excrement-disposal. In fact the cities of Japan are the only cities in the world that have suffered less from the effects of human ordure than the capital of Victoria—during the last five years. The statistical proof of this assertion I shall produce at some future time. In the meanwhile the reader must use his own discretion as to accepting or rejecting the statement as it stands.

340. It may be added, however, that one great source of typhoid poisoning—that by water—has been cut off from Melbourne. Had the place been dependent for its water-supply upon wells, or underground tanks, the probabilities are that the fœcal matter in the soil of the city, at the time when typhoid fever was so rife, would have found its way into many of the wells and, owing to the continual additions of fœcal matter, they would probably have remained polluted for years. This mode of the propagation of typhoid was, fortunately for Melbourne, precluded by the river Yarra in the first instance and by the Yan Yean Reservoir of late years. The Yan Yean water-supply is as perfect as the water-supply of any city can be—so far as the chances of pollution by zymotic disease

germs are concerned. The water contains a larger proportion of vegetable matter and earthy particles in suspension than is altogether pleasant to the eye, or to the taste, and great complaints have been made by the citizens on these accounts. But so long as the in-flowing waters of the reservoir remain as free from fœcal pollution as they have hitherto been, the Yan Yean service-pipes will not be the distributors of poison germs. There was great alarm some years ago about the drainage of Whittlesea, which, it was said, was gravitating towards the Yan Yean. The matter was investigated, I believe, thoroughly, and the levels taken showed that the sewage of the village could not reach the reservoir. The instinctive dread of the citizens of Melbourne at the possibility of such a thing, however, as shown by the out-cry raised, was a good wholesome sign.

341. I am reminded by the statement made in the last paragraph but one that I have possibly created a wrong impression by some former remarks as to the excrement-sodden condition of Melbourne [4]. I mentioned that it was by reflecting on the accumulated and cumulative organic matter in the soil of Melbourne and the decrease in dysentery at the same time, that I first saw that alluvial decomposition was not a sufficient explanation of the causation of the disease. This is literally the case. But I was not aware at the time the idea occurred to me, nor indeed at the time I wrote, that the excrement-disposal system of the city had undergone a change, and a material change, within the last few years. I may as well confess that I—an old resident of Melbourne—had not taken note of the fact that the system of privies had been condemned, that the old shafts had been filled up as fast as the means and position of affairs would admit, and that a weekly night-soil removal system had been substituted. I admit I was ignorant of the check that had thus been given to the underground fœcal saturation of the city. It is, as I learn now that I have had my attention drawn more especially to the subject, not yet absolutely stopped, but the work is proceeding as fast as practicable. However, the views I arrived at are not substantially affected by this modern improvement. Let it be granted that the innovation has been in effective operation for the last four or five years and there would still be 13 or 14 years during which the soakage was progressive and cumulative. It is only to be added that the night-soil now removed from the city is dug into ground in the vicinity to such a depth, that its perfect deinfection is insured, whilst all malodorous emanations are prevented.*

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* There is one little practical suggestion I would throw out for consideration. The rattling and rumbling of the night-carts is a serious evil, more especially in those parts of the city to which they converge nightly. Could not the wheels of the vehicles be provided with india-rubber tires? And might not the horses be shod with leather? Some contrivance of this kind seems required to abate the nuisance of the noise made by the strings of carts perpetually rolling through some of the streets in the neighbourhood of Carlton. It is bad enough to be roused by the din and clatter made in the rights-of-way and at the back of the houses in all parts of the city once or twice a week. But it must be intolerable to those living on the outskirts to have this sort of thing going on every night.

342. Lest it should be supposed that I intend to cast any reflections on the excrement arrangements at the Yarra Bend Asylum, by exhibiting the number of deaths from dysentery in that establishment in strong contrast to those in other parts of the city and suburbs, it must be remembered that the conditions surrounding the insane in such a building, or such a group of separate buildings, with their numerous yards, gardens and grounds, are highly causative of dysentery in this climate. For practically there must always be—notwithstanding vigilant care and supervision—great danger of surface pollution. Unfortunately a large proportion of lunatics have delusions connected with their excreta; and, in such an extensive institution, I suppose it is almost impossible to provide against some exposure of excrement to the influence of the air—the more especially that many of the insane and the idiotic conceal their ordure in holes in walls, in the clefts of trees, and in the crevices of the large stones cropping out of the ground. This horrible phase of insanity has probably led to many deaths from dysentery in the past, and may yet lead to many in the future—unless perhaps the danger is to be obviated by some ingeniously contrived system of arrangement and classification, by which those afflicted with this symptom of mental disease might be brought under more special observation. In any case, however, the persistent, ever-watchful, cunning of the lunatic will, in this as in everything else, be more than a match for the management in some instances. So it is much to be feared that isolated cases of dysentery will always be imminent in all Lunatic Asylums in warm latitudes in every part of the world.

343. To show that the general excrement-disposal system of the Yarra Bend is not faulty, I may mention that, as far as I can learn, there has not been a case of typhoid fever in the Asylum—certainly not a fatal case—for the last 10 or 12 years. This is quite sufficient evidence to my mind that the ordinary, or general, excrement-disposal system of the Institution is a safe system, and that the excreta are perfectly deinfected—so far as the Asylum is concerned. The water-supply is the same as that of Melbourne—from the Yan Yean Reservoir.

344. Although there may be no absolute occasion, perhaps, to show that the dysentery of Melbourne is true dysentery, and not mere *colitis*, still I have thought it better to place the matter beyond the reach of doubt. I have therefore extracted from the evidence given at Coroner's Inquests at the Yarra Bend Asylum such portions as refer to this subject. I think few will venture to question the fact that these were cases of malignant dysentery:—

At an inquest on Joseph S— taken at the Yarra Bend Lunatic Asylum on the 18th May 1868, the following evidence was taken:—

John Bayldon M.D. on his oath said as follows:—"I am Resident Medical Officer at the Yarra Bend Lunatic Asylum. I find from the records of the Institution that the deceased Joseph S— was admitted January 12, 1865, under the warrant now produced. He is described as being then aged 35 years; suffering from

General Paralysis and, after his admission, liable to attacks of Dysentery. I saw him first on the 10th November 1867. * * He was again attacked with dysentery, for which he was re-admitted to the Hospital, on May 5th 1868. I prescribed medicines both by the mouth and by the rectum; and medical extras; but they had little effect in checking the disease." * * *

James Thomas Rudall on his oath said:—"I am a Fellow of the Royal College of Surgeons of England. I have made a *post mortem* examination of the deceased J. S—. Externally * * * (Conditions of all internal organs stated.) The stomach was healthy. There was a very small quantity of brownish fluid in it. The small intestines were healthy. The large intestine from the cœcum down to the rectum exhibited the changes found in dysentery—thickening of the intestinal walls, ulceration, sloughing, and the contents a dark, dirty, grumous fluid. The cause of death was dysentery."

Verdict—That "on the 15th of May 1868, in the Yarra Bend " Lunatic Asylum, where he was in legal custody as a lunatic, " J. S— died from dysentery."

Inquest on Edward L— at Yarra Bend, 21st November 1868:—

Dr. Bayldon said:—* * * "He came under my charge on the 11th instant. Dr. Gordon reported to me that the patient had been under treatment for Dysentery for several days, and that the disease was assuming a very unfavourable form. The deceased had very severe diarrhœa with much blood in the stools. He had almost constant sickness and vomiting. I prescribed ice and soda-water," &c., &c.

William Henry Cutts, M.D., made the *post mortem* examination. Omitting irrelevant details the following occurs:—"The large intestine was disorganised in its entire length and broke down in several places in removing it from the body. Its mucous membrane was for the most part thickened and softened, but in places entirely destroyed, and its entire surface covered with ulcers and sloughs of all sizes and in all stages of degeneration. The mucous membrane of the lower portion of the ileum was intensely inflamed and towards the ileo-cœcal valve showed large sloughing ulcers. The corresponding portions of the mesentery were deeply congested. The cause of the death of deceased was dysentery."

Verdict accordingly.

Inquest on Benjamin M—— at Yarra Bend, 7th April 1870.

Dr. Bayldon. Deceased "continued in his usual state until the end of last month when he was attacked by dysentery," &c.

William Godfrey Howitt made p.m. "On opening the intestines I found extensive ulceration all over the ascending, transverse, and descending colon. There was a perforation of the colon just above the right kidney—but the contents had not escaped. The cause of the death of the deceased was chronic dysentery."

Verdict as before.

Inquest on Samuel T—— at Yarra Bend. 9th December 1872

Patrick Smith. "I am resident Medical Officer. * * * On the 29th November he had slight diarrhœa and seemed to become quieter. He was ordered, &c. * * * On 3rd December dysentery set in and he was sent to hospital. * * On the 5th December I saw him in consultation with Dr. Paley. On the 7th December he seemed to rally a little, but on the 8th he had a relapse. I again saw him with Dr. Paley on this day; but he died at 1 a.m. on the 9th December. * * *

Frederick Thomas West Ford made the autopsy. * * * "The colon was thickened and ulcerated, and some of the ulcers had nearly perforated. The small intestines contained fœces and were much congested. * * * The cause of death was exhaustion from dysentery and inflammation of the peritoneum."

Verdict—dysentery.

345. The reference to the Chinese at present in Melbourne requires to be supplemented. I am not prepared to say that these people do actually contribute more than their proportion to the returns of deaths from dysentery in Melbourne and its suburbs. Their excrement-disposal system so far as Melbourne itself is concerned is theoretically perfect—though practically disgusting to an Englishman—as an inspection of the Chinese Quarter in Little Bourke Street will soon convince the visitor. The plan adopted is that of receptacles, placed in rooms and on stairs, or landing places, and in passages. At certain periods these receptacles are emptied and the material is removed by the Chinese. If the system were genuinely carried out and no interruptions occurred, it would no doubt be perfectly safe as regards the city; and as the effluvia from the arrangement only affect the Chinese themselves, it does not much matter. But accidents may occur and some of these receptacles may be upset, or left for a long period—long enough for the fluid contents of the vessels to evaporate. What might happen in either of these events it is difficult to foresee, but it may be conceived possible that dysentery or typhoid fever might be generated. The chances of such a thing are no doubt lessened materially by the sobriety of the race; but then some of them smoke opium. Nothing obnoxious has come of the mode in which the Chinese deal with their excreta up to the present time; or at all events it has not been complained of. It may therefore be a remote contingency; and at all events the system is a far better one than the system of the permanent retention of the deleterious material in the city. The present plan has fewer objections than that would have. But the question now comes of the ultimate destination of these excreta. I assume they are used somewhere near Melbourne for manurial purposes. If so, I should expect that the Chinese gardeners would conduct their operations as in China. And seeing the result there, it may not be an unwarrantable inference that a similar result on a smaller scale might accrue in some of the environs of the city. Some of the

market gardeners of Brighton and its neighbourhood commit the error, as was pointed out [141], of leaving night-soil on the surface of the ground; and contract dysentery, or typhoid fever, in consequence. The Chinese are not unlikely, it seems to me, to do the same—though I have no data to go upon.

346. In concluding my remarks upon the subject of the dysentery of Melbourne and its suburbs, I say without hesitation that although isolated cases of the flux will, almost of necessity, occur, from the conditions of climate and mixed population; yet that such epidemics of dysentery as ravaged the city in 1853, and again, though with a less mortality, in 1854, are an utter impossibility—so long as the surface cleanliness of the place is kept up to the present mark. I speak of the external cleanliness as regards fœculent matters only and I limit my remark to epidemic dysentery. How far large values of other putrifying organic material are concerned in the causation, or propagation of other diseases, I have no means of judging and I do not venture to offer an opinion. But with regard to dysentery I feel myself justified in saying, on the strength of deductions arrived at from a vast collection of facts, that in no city in the world where present English habits and customs in the matter of public cleanliness and the outward observance of decency are enforced, can an epidemical outbreak of the flux occur. Melbourne was deluged with dysentery simply because she was overwhelmed by the hordes which suddenly invaded her and led to her temporary demoralisation.

347. Before quitting the dysentery of New Holland, I will draw the reader's attention to the short notice of the country in Dr. Parkes's Practical Hygiene. In that part of the work treating of British troops on Foreign Service, will be found [P. 576—Section X.] a brief statement that will go a long way to confirm the views expressed in these pages. Dr. Parkes says:—"It seems unnecesary "to describe the climate of Australia. * * * These "countries *at present* are known to be very healthy; this arises in "part from the absence or great infrequency of malaria." After a tabular statement of the admissions of troops in 1862, from diseases among which dysentery does not appear, Dr. Parkes adds;—"and other smaller items, no disease being of any gravity. "It only requires a glance at these figures to show, not only the "healthiness of Australia, but that a little individual management "and good conduct would remove much of this sickness."

348. The words *at present* in the above quotations I have italicised, inasmuch as they seem to point to a sort of innate feeling on the writer's part that although the country may be very healthy now, the time may be coming when Australian communities, following in the wake of older civilised countries, shall succeed in engrafting malaria on the soil. Whether Dr. Parkes implied as much as this I do not know, but the manufacture of malaria by nations themselves is, to my mind, a logical certainty—as great a certainty as the fact that malaria in particular localities has been unmade by nations.

DYSENTERY AS OBSERVED IN VARIOUS PARTS OF THE WORLD.

349. If now we turn from Australia, we shall find there is scarcely a spot on the earth's surface where dysentery does not appear; and where the direct connection between surface-pollution and the flux is not easily to be traced. And, moreover, we shall find that in those limited spaces within which epidemics never occur, and where sporadic cases are rare, the general excreta-disposal system of the country precludes the exposure of large quantities of fœcal matter to the action of the air. First let us briefly consider the countries which have an analogy to New Holland. California presents the closest possible parallel. There dysentery was as much a constant at gold-fields as it was afterwards in Victoria. The experiences of San Francisco too were those of Melbourne; and the gulches of the mining districts produced the exact equivalent of disease that the gullies and flats of this country brought forth. The tendency in California however was rather to typhoid than to dysentery. The flux was not nearly so terrible there as it was here; both because the numbers of miners collected at one spot were not so large, and because the local conditions were not so favorable to the evolution of the dysentery germ. Californian mining on the whole was more nearly allied to Woods' Point and North Gipps Land mining. There would appear to be something in altitude which modifies the effects of surface-pollution in some way. I do not profess to explain it, but there is the fact. Dysentery is certainly not so frequent in mountain ranges as on plains. And even although it does occur in warm vallies at a considerable elevation, it does not spread beyond, so far as I am enabled to learn, or to judge, from the accounts we have of the disease. California, however, brought forth dysentery largely whenever and wherever the momenta were brought to bear. I have not yet been able to get at the complete dysentery history of the State of California; but I shall look to find that the country passed through very nearly the same phases of infection that this colony of Victoria has passed through. I infer that the capital and large inland towns have scavenged the disease away, but that isolated cases are found in them, and that more wide-spread endemical attacks occur at "rushes" as they do here.

350. The only connected account I have seen as yet of the Cape Diamond Fields, is that given by Mr. Frederick Boyle in his book —"To the Cape for Diamonds"—published this year. The position of affairs on the diggings there is clearly very nearly the same as it was here in the early days. Mr. Boyle's sketch of Dutoitspan vastly resembles any one of the old diggings. "Vile smells assail "the nose. An utter recklessness of decency is one of those camp "features which most speedily impress the visitor." The mining for diamonds leads to a similar disposition of the earth that "surfacing" and shallow sinking for gold used to end in. The diamond seeker has his claim of so many feet and he either sifts

and sorts the surface, or puts a shaft down to the "diamondiferous" strata, according to the lay of the field. As some of the shafts are sunk to a depth of 40 and 50 and even 60 and 70 feet, it will readily be understood that these diamond mines have pretty nearly the same physical aspect that old Golden Point at Ballarat had in 1851.

351. The first outcome of these conditions is given at p. 163. "We had a sensation at Bultfontein to-day of the most unpleasant "kind. A Kaffir boy in Mr. Webb's service fell dead across the "kitchen threshold: he had been ill of a slow fever about two weeks "past, but no one had thought the matter serious. * * * "My attention thus abruptly called to the sanitary condition of "the camp, I find the air to be full of grewsome rumours. The "belle of Dutoitspan lies dying of violent typhus; a woman was "brought in from the veldt this morning, dead long since; a well-"known digger has died to-day; another young lady is given up." Mr. Boyle looks into the matter and finds the "ghastly tales of "disease prove to be," as he expected, "all moonshine." The return of the Registrar given at p. 171 certainly does not quite bear out the idea of an epidemic. From Nov. 22 to Dec. 4, 29 deaths are registered—17 of which took place during four days of December. The registrar commenced to register deaths for the first time on Nov. 22. "This," says Mr. Boyle, "is no startling "catalogue of deaths in a population of 15 or 20 thousand, many "of whom are of dissipated habits, with constitutions undermined. "Out of 29, 16 are children; 3 more have died by accident, and "another is a girl. This leaves but 9 grown persons who could "possibly be reckoned victims to the climate; and I know that "several of them were notoriously loose livers."

352. This catalogue may not have been startling to the writer—it does not sound very heavy in damages to a barrister perhaps—but it has its grim side nevertheless. At all events the previous rumours can hardly be called "all moonshine." For if we take the death-rate for the four days of December as a basis on which to calculate the annual rate, we find that it gives over 1500 deaths for the year in a population of 15 or 20,000—rather a serious mortality. If we add to the deaths formally registered, the deaths which were not heard of—the deaths of poor wretches left to their fate in tents by themselves—the deaths which the miners would not take the trouble, or lose the time, to report to the registrar—the deaths of those whose friends did not know of the initiation of a registration system in the district—if we add all these to the catalogue, the death-rate might, I suspect, have warranted some of the grewsomeness of the rumours—even although Dr. Gibson says, "allowances made, it will bear comparison with "the average of Continental towns."

353. Among the causes of the deaths, I find Fever 8; Dysentery 5; Unknown 4. Among the deaths from fever, 3 were of children; among those from dysentery, 3 were of children; 1 of a boy of 12 years 6 months; and 1 of a man of 52. The next mention

of sickness in Mr. Boyle's book is in chapter xxiv. :—"I think "there is more sickness just now than at the time of my arrival. "But it is a trifling matter. Beyond a doubt, these fiery, but "open plains are amongst the healthiest in the world. We have "everything to contend against: reckless disregard of sanitary "rules on the digger's part, &c. * * * The mortality "considering all things is astonishingly small. But nine died "last week, of grown men, whether by accident or disease, in all "this camp, of no one knows how many thousand inhabitants, "tent-dwellers, in an atmosphere polluted by open wells of filth, "and carcases decaying. Children suffer more, as would be "expected, but there do not appear to be so many of them as formerly." In estimating the population Mr. Boyle sets it down at 11,500—the Government making it 15,000. "Many are leaving; "some to enjoy their hard-earned wealth, some in disgust, many "ill, and many in fear of illness." Mr. Boyle "left the fields for "good" on the 7th of March 1871—having arrived at the Camp of Pniel on the 19th of November. He has nothing more on the sanitary condition of the diamond fields.

354. It is extremely hazardous to arrive at conclusions upon inexact data of this kind—the more especially as it is evident that Mr. Boyle takes rather a light and airy view of sickness. His account, however, is graphic and valuable in many respects—as a descriptive work, by the way, the volume is picturesque and spirited throughout—and I think it will support the following deductions :—(1) That "these fiery, but open plains," which "are among the healthiest in the world," were free from malaria until they were occupied by the diamond miners. (2) That dysentery and typhoid fever were present on the fields. (3) That these diseases were either introduced, or engendered, by man. (4) That although there was not a sufficient amount of sickness, or so large a number of deaths, as to constitute an epidemic, yet that the death-rate was excessive. (5) That the extreme dryness of the climate may account in part for the comparatively small number of disease germs in the air.

355. As regards the first deduction we have not only Mr. Boyle's direct assurance of their healthiness, but there is collateral negative proof in his book. There is not a word of ague or remittent fever anywhere; nor is there any mention of illness except where the mining populations are in force. Besides, the description of these large plains, or veldts, treeless, waterless and soilless wastes, with a blazing sun and a crisp air above them, precludes the supposition of malaria. As therefore the ague-plant was not present, and there were none of the quadrumana in the neighbourhood, it appears to me a safe conclusion that these open veldts were absolutely free from miasms and malarias deleterious to man.

(2) That dysentery and some form of fever were on the field at Bultfontein seems pretty clearly established. If the dysentery had been confined entirely to the children its true nature might have seemed doubtful; but the death of a man of 52 makes it more

able that the cause of death assigned in the registrar's s correct. I assume that the fever was typhoid, and not r obvious reasons which need not be specified. The eath of the Kaffir boy, by the way, and the previous the fever, look as though he had that type of typhoid yphus *ambulatorius*—ending in perforation.

e inference that the flux and the fever were generated *de* s to be almost forced. It is difficult indeed to see how ble for them to have been transmitted some hundreds to the interior of Africa—a distance which it took Mr. n days to traverse, travelling day and night by the best veyance. It is possible to suppose that they might have mitted from the nearest farms of the boers—but the is not sustained by anything in the book, or by the nature of the diseases. [As regards dysentery, indeed, t of the extremely limited number of cases is strong e non-contagiousness of the affection, and is, of itself, to those who believe in the spread of the flux by con- *fomites.*] In fine the conclusion is forced that both re endemic.

e was clearly an excess of typhoid fever for the popula- The want of humidity seems to be the explanation of amount of dysentery. All the other conditions were sent. I confess, though, to being somewhat surprised ention of a previous epidemic the year before. For I t there must be continued moist weather at some period r. When the hygometric state of the air reaches the point he production of the dysentery germs is favoured, and that point for a while, I should most undoubtedly ex- s epidemics of the disease to break out on the diamond r the other factors are evidently there in abundance; breaks of the flux like that in the Dutch encampment Dr. Lichtenstein [17], and like those at gold mining not take place in the interior of Africa, my propositions ropagation of dysentery will have to be enlarged or modi- disease itself being there, I do not at present see why, under rently favouring circumstances, it should not have spread air became moist. However Mr. Boyle was present lds only during three or four months of the dry season, ot unlikely that the events of the preceding year may forgotten, while he may not have heard the result of the ther which seemed to be setting in when he left. The t at present stands, is somewhat opposed both to my causation and to the propositions as to propagation. I willing to admit that it tells against my views so far. e stated it in full confidence that when all the facts shal they will square with those views. If not, the views are ogether wrong, or they will have to be amended so as to henomena which, as far as I can ascertain, have not been elsewhere. If it be shown eventually that the concurrence of extensive surface-pollution with a moist state of the atmosphere has not resulted, and does not result, in copious dysentery at the diamond fields of Africa, I can only say that the absence of the disease, under such conditions, is very remarkable and unique. That there was rain at Dutoitspan while Mr. Boyle was there, by the way, is clear, for the fact of Kaffirs and others hunting for diamonds in the mud round the tents is mentioned. That there should have been so little dysentery where so much excrement was exposed, is, to me, unaccountable. I can only suppose that the thunder-storms were succeeded by such extremely dry weather at once, that the greater quantity of moistened fœcal matter was hardened exteriorly again before the dysentery germs could form in large numbers. The absence of trees, the hot sun overhead, and a scorching wind, would quickly arrest the development of the germs no doubt. But if the rain was followed by anything in the shape of sultry weather, or dew at night, the production of myriads of germs should, theoretically, have been the result. The point may be cleared up some day.

356. Of British Columbia I know little; and I have not had time to procure the statistics of the country. A Melbourne barrister, who was on the Carriboo diggings, informs me that the miners there suffered from what was known as the "Mountain Fever," of which many died. As the altitude of these diggings is about 5,000 feet, and as the excrement disposal system is the same as that of these fields, I assume this "mountain fever" to be typhoid. There was no dysentery there. This corresponds exactly with the result of surface-pollution in mountainous districts here. I could not learn whether dysentery appears at the mining camps at lower elevations in the summer or autumn, but I should suppose it does.

357. The subject of altitude, in its relation to dysentery, may possibly receive some elucidation from Switzerland, where as far as I can determine at this distance, the same phenomena of the suppression of the flux and the production of typhoid, will be found. The Swiss utilise night-soil as manure and leave it on the surface of their fields. The outcome is of course, at certain epidemical seasons, in certain places, a fruitful crop of typhoid. But I cannot learn that dysentery ever makes its appearance in the mountains of Switzerland, even though it may be raging all round. On this point, however, I have nothing more than negative evidence.

358. With the rest of Europe I may deal collectively and briefly. There is not a nation subject to the visitations of dysentery epidemics, in which their appearance may not be associated directly and evidently with surface-pollution. The whole continent of Europe—unless perhaps Holland of late years, and Belgium, though I doubt it—is still in the position as regards excrement-disposal that the English were 150 or 100 years ago. I do not mean absolutely in the same position; for water-closets and privies were unknown in England at those early periods, or were not largely intro-

duced, whereas they are extensively used now everywhere abroad. But the introduction of these conveniences has not been universal among the masses of the people; and, moreover, it must have been palpable to everyone who has travelled that the water-closets on the continent—at all events those open to travellers—are not used as Englishmen use water-closets. In fact the habits of all classes in matters connected with the natural functions show not only a disregard for scrupulous niceness, but a tendency to downright uncleanness. Foreign nations ridicule the disinclination, amounting to abhorrence, that the English have to refer to these things, and consider it a *mauvaise honte*. And perhaps the delicacy of feeling may, as they allege, be carried so far in some rare instances as to lead to serious physical injury to individuals. But at all events dysentery epidemics cannot occur among people who have a wholesome sensitiveness on these points. The national observance of suppressing all mention of, or even distant allusion to, these subjects may have its occasional inconveniences possibly, yet the general tone that has prevailed in consequence has proved an eminently healthy one for England. The key-note at last struck by the higher classes, was by degrees taken up by the successive grades of society; and though getting fainter and more faint towards the lowest, it has yet been sufficiently heard to prevent a continuance of general surface-pollution, and to cut off one source of disease that overwhelmns the other nations to this day.

359. When the rulers of the world shall gradually come to learn, as they will, that their subjects have been perishing for centuries from plagues and pestilences which they have brought upon themselves; and that millions now die annually simply from omitting to deinfect their own excreta; they will each find it a difficult matter so to educate their own people as to wean them from their deadly habits. Generations must pass away before a thorough belief in, and an adequate dread of, the poisonous material can be established. Better therefore an over-refined tone of public feeling which prevents the masses from soiling the face of their country, than a prevalent easy familiarity which, knowing of no shame, leads to open violations of decorum and to a filthy condition of towns and cities. Perhaps, however, something may be done by ordinances and decrees towards cleansing the continent of Europe. It will be up-hill work to fight against immemorial usage and prescription :—not quite so hard as to do battle with Hindoo races, it may be, for the religious element does not step in; but yet there may be awkward complications. The Roman citizen has a common law right, it would seem, to micturate, or defecate, within the gates of any Palazzo, or anywhere else in the Holy City, at any time. If he selected the most crowded thoroughfare in broad day no one would interfere. The same of course in the other cities of Italy. A sudden or violent interference with such a right as this might be cause of bloodshed. The lazzaroni of Naples might find another Massaniello. But better a few hundred lives lost in that way than the constant,

steady, wholesale, empoisonment of the nation. It would be well if the crime of exposing excrement in the midst of populations came to be looked upon as one of deliberate murder, or murder at large. I am satisfied that, as a matter of fact, it is certain death to a large proportion of those who are brought within the sphere of the operation of the subtle virus it breeds.

360. Since certain quarters of the large towns and cities of Europe are permanently more or less in the same state of pollution as the temporary encampments of mining communities, there is no occasion to demonstrate that they will be liable to similar outbreaks of dysentery and typhoid fever, more or less extensive, in proportion to the quantities of fœcal matter exposed and the local and seasonal conditions present. I therefore pass on to other regions. Egypt is not a bad field for viewing the connection of fœcal matter with dysentery. The habits of the people there are quite sufficient to account for it, and for all the pestilences from which it has suffered, and from which it will suffer. I observe, in a review of a recent work, "Cairo and its Climate," by Dr. Moritz Fürstenburg, that the country has its periods of freedom from dysentery. Dr. Fürstenburg says:—"Epidemic dysentery seldom occurs in the winter"! What a relief! In deprecating the fears of those who hesitate to resort to Cairo on account of the Ophthalmia, or "Blenorrhœa of the Eyes," he points out that "Egyptiatica" is "unknown in clean houses or hotels." Here, patently, is another glorious country turned into a vile abode of malaria through the malign influence of man. Your eyes are guaranteed to you—if only you restrict yourself to *clean* houses! And if you go to Cairo in winter you may perhaps get in between the epidemics of dysentery! Get away from the Egyptians anywhere in Egypt, in fact, and you are safe.

361. There are certain tribes of Arabs who are said to have such scruples in connection with one bodily function, that it is reckoned a disgrace to be supposed to fulfil the office at all. No male of the tribes is ever known to defecate and the evidence of the fact is hidden in some secret place. They follow out the Mosaic ordinance with more than care—with a fastidiousness that throws the English into the shade. Women and children only are pardoned for the offence of being suspected or found out, because of their helpless position; but for an Arab adult male of these tribes to lie under the suspicion, is a mark of womanliness, or childlishness, that is an indelible stain upon his manhood. Is this an authenticated fact? And is the freedom of the interior of Algeria from dysentery and other zymotic diseases to be attributed in part to their singular custom? The French may be able to give some specific information upon the point, as well as upon the question of the absence of dysentery among the warlike tribes with whom they fought. I cannot find any reference to the flux among the Arabs and I am under the impression that they never generated the malady. I speak of the inland races—the tribes led by Abd-el-Kader. The seaboard is infected everywhere.

362. The Turks are fearful sufferers from dysentery, as well as from typhoid fever and bubonic plagues. Whatever observance Mahomet may have enjoined upon his followers in the way of sanitary precaution, certainly they are not observed at Constantinople. The pollution, both above and below the surface, is sufficient to account for the presence of all the diseases and for the periodical support and transmission of the cholera germ. Gibraltar, Malta, and every island in the Mediterranean, supply instances of surface pollution and its resultants; and, moreover, the history of the places which are under British rule, here, as elsewhere, furnishes marvellous confirmation of the truth of the proposition that malaria is an artificial product everywhere. The decrease of disease has been in a direct ratio to the diminution of excrement—as may be seen at a glance by a reference to Dr. Parkes's valuable work on Practical Hygiene. I may in fact spare further remarks on British possessions in any part of the world and refer the reader to it for information. He will find in every region where we have a garrison, abundant evidence to account for the prevalence of dysentery and other zymotic disease, both among the civil populations and the troops. Opening these stores of hygienic knowledge with the key of fœcal pollution, the variations, fluctuations and disappearance of dysentery are as plainly to be read, as though the dysentery germ were at this moment demonstrated.

363. Russia has been scourged by dysentery epidemics for centuries; and typhoid fever had nearly proved fatal to the Czarewitch the other day. The occurrence of these diseases is easily to be accounted for on my view; but I refer to Russia now for the express purpose of pointing out, that since her criminals are sometimes handed over to scientific men for experiments which may or may not result fatally, I know of no more excusable object than that of bringing out the causation of dysentery by such means. The question of the origin of the disease and that of its exact infective agent, might very quickly be set at rest in Russia. And there is no question of such magnitude to be named at the present day. For, as I have said, a mildew on excrement once proven to be the cause of dysentery, other mildews on excrement must, inferentially, be the causes of typhoid, cholera, yellow fever, intermittent fever, relapsing fever, plague, &c., &c. Given a dysentery mildew and there cannot be any other than a mildew cause for the other diseases in the catalogue. Therefore, if any object may be thought to warrant the risk of human life in cold blood, certainly there could hardly be found one of such vital interest to the world as that of determining the dysentery germ—or the typhoid germ. For it matters not which of all these germs is the first to be discovered. If it be found on excrement, the others will be found on excrement.

Russia is one of the main channels along which the cholera wave pours into Europe. In St. Petersburg too they even preserve the cholera germs in ice and thaw them in the winter, so as to induce small local epidemics. All this bespeaks a highly faulty excrement-disposal system, and an artificially created malarious condition. Of the gold-fields of Siberia and the dysentery there I have learnt nothing. Probably the hot seasons are prolific of the disease; though it may be that the rules under which the gangs work disturb the conditions of germ formation.

364. The Persians, or those in Teheran, do not appear, from all I can learn, to have been subject to dysentery epidemics; though the present plague of typhus, or typhoid, argues a present defective excreta-system. From one who was some time in the Persian capital, I learn there is a peculiar arrangement there. All the houses have closets for the evacuations, which find their way into shafts of very great depth sunk in the soil. What becomes of the night-soil eventually, I could not ascertain; for my informant had been impressed only with the singular fact of the depth of these privies. The poorest classes within the mud walls of Teheran have privies and there was no notable surface-pollution. The late famine, however, may have changed all this, by leading to a concentration of people in and around the city; and people demoralised by starvation are reckless as to personal or other cleanliness. This is the explanation I take it of the production of famine plagues. The want and crowding are inefficient of themselves to produce a specific germ; but when some of the people become too weak to attend to their discharges themselves, and the others do not trouble themselves to remove them, then the germs form with a rapidity, and in quantities, proportioned to the conditions, and to the amount of fœcal matter. Although dysentery has not been alluded to as an accompaniment to the Persian plague, as yet, I fear the conditions brought about by the fever will eventually end in this terrible malady being engendered.

365. The history of dysentery in North America is precisely that of British communities transplanted to a new country and frequently receiving large infusions of foreign elements. The cities and towns, where the surface cleanliness is as nearly that of English cities and towns as a mixed population will admit of, are free from dysentery epidemics as a rule; though in the lower latitudes, where the conditions are favourable, small outbreaks of the flux occasionally happen. In the country and on the outskirts of civilization, the disease presents the same phases as in Australia and elsewhere. In the autumn, or fall, dysentery and typhoid [or "fall fever"] generally make their appearance—the former of course being attributed, as it is almost everywhere, to the diet and the alternation of hot days and cold nights with heavy dews. A very little examination, however, will, I feel sure, suffice to convince the Americans that where dysentery is, there surface pollution must be. And the converse of the proposition will follow. Their "fall" fever, too, they will discover to be generated by themselves; and that malaria from climate is as great a bugbear in America as in Europe. Excepting the "ague-plant" discovered by their own countryman—to whom no doubt they have rendered all honour—they will find there is no indigenous disease germ born

of the soil. If all the human excreta of America could be disinfected and deinfected to-morrow, and after, I submit that their zymotic disease mortality would be very trifling in the future. The country is healthy and wholesome enough in every part, except the ague swamps, if only man would not poison it.

366. This reminds me of the occurrence of a disease in New Brunswick usually associated with tropical regions. I refer to the extraordinary appearance among the French there, some years ago, of Leprosy or Elephantiasis. There was an immense deal of investigation into the subject by the learned; and the general conclusion was that the disease was engendered amongst the French colonists by their fish diet and by the use of stoves. The affection it appears was limited to the French—the English population not contracting this loathsome disease. This singular outbreak of leprosy, with its rigid line of demarcation between people of different nations in the same country, does not seem to me to be fitted with an efficient cause, by a combination of fish diet with heated air in the houses from stoves. Something more must be assumed. For although a high temperature seems to be a required condition, the large or frequent use of a fish diet does not appear to be a necessary condition in other countries. It is not a constant. And the law of causation in specific poisons, as I understand it, is—once excluded, never included. Fish and stoves, therefore, do not carry conviction to my mind. And I suggest that the difference in the modes of excrement-disposal between the French and English underlies this question of causation. I suspect that the French placed themselves in an analogous position to the natives of tropical regions, by creating great artificial heat and by allowing their excrement to remain within its influence, so that the poison germs, whatever they are, of leprosy, were engendered. The English did not retain their excrement within doors.

I do not say the hypothesis is complete or will cohere in all its parts. I throw it out as a suggestion which those who are interested may take up. If the surmise now hazarded prove correct, and if the matter on further investigation should lead to the discovery of the causation of leprosy, it will be a curious thing that a disease of hot climates should be elucidated by its accidental occurrence at New Brunswick. It will also illustrate the terrible nature of that poisonous material which has been so long unsuspected of playing the part it does. And I know of no assumed cause of leprosy that so commends itself at the present moment, as an exotic fungus on excrement—hot-house forced in New Brunswick. The cholera germ I take to be revivified, not generated, in a similar manner in St. Petersburg.

367. The dysentery of South America is wide-spread. There are few towns that have not been visited by its epidemics at some period or other. But from some unexplained cause the flux of South America is not nearly so formidable as that of the East Indies, or of other countries. The mortality is not one half, it is said. The frightful attacks of the yellow fever in its various

result of immemorial usage: while the only concession to public decency the Roman lower orders feel themselves called upon to make when pressed is to step within the nearest open doorway. The French have undoubtedly got beyond this stage. Their gentry would not endure, and the lowest grades would not think of, such bestiality. But yet it cannot be gainsaid that while the surface-pollution of France is far less than that of Italy, it is nevertheless far greater than that of England. No doubt the immunity of Paris from epidemic dysentery for the century preceding the year 1859, was due to a reform in manners similar to that which had freed London from periodical attacks. But though I am not prepared with the precise explanation of the mode by which the surface-pollution that led to this epidemic of 1859 was brought about, I say with confidence that a thorough investigation of the circumstances preceding and attending it, will show that surface-pollution was the material element in the causation. If all England were ablaze with dysentery at this time, I maintain that an epidemic could not possibly gain a footing in London—unless it became so crowded that the present sanitary arrangements were not sufficiently elastic to depurate the additional excreta.

371. With regard to the comparative mortality of dysentery with other diseases [*b*], I have already given my reasons for concluding that dothinenteria (typhoid, or enteric fever), has been on the whole more deadly, as it is certainly a more formidable malady to guard against, than dysentery.

372. Trousseau asks "what are the causes of epidemic dysentery?" [*c*] And he answers it by saying that "they elude our observation." I trust and believe that the microscope will soon bring them clearly into light.

373. The problem [*d*] of causation put by Trousseau as to the occurrence of dysentery at Tours looks difficult at first sight. Yet though I shall not attempt to solve it, as I do not know the exact relative conditions of the cavalry and infantry barracks as regards their excrement disposal system, I am persuaded that a little local knowledge would give the clue. Trousseau says "The same hygienical system is adopted in both" barracks; but this is too vague. He probably refers to the drainage and sewerage, and to the ultimate destination of the excreta of the barracks. Not having the question of the surface exposure of excrement present to his mind, it would not occur to him to examine into the relative conditions of the latrines. The modes of emptying these, and the periods of emptying them, and the disposal of the material, may have been precisely similar in both cases. But there is another and far more essential thing—the formation of the latrines themselves—to be taken into calculation. These *lieux d'aisance* may vary considerably in their configuration and in their internal arrangements. Without knowing anything of the facts, I predict it will eventually transpire that the fœcal material in the cavalry latrines is, from some cause, more exposed to the external air, or placed under more favorable conditions for the generation of the dysentery germ, than in the infantry latrines. A very little variation in these conditions may

make all the difference between the production, or non-production, of the germ. I can conceive it quite possible that a different aspect even may be the turning point in the formation of a mildew. So that until these questions as to the interior economy of the two latrines are further and more minutely looked into, I cannot concur in the conclusion of Trousseau that this is "a case in which no charge can be brought against the local situation," or "the hygienical conditions." It may be that the cavalry quarters are more exposed to the effects of surface pollution outside the barracks, than the infantry.

374. The subject of fruits [*e*] may be passed over. The "something we call the *epidemic constitution*" [*f*] is something I do not understand. It appears to me to be one of the convenient phrases current in scientific diplomacy. The other portions of these paragraphs require no special attention. The "exceedingly contagious" nature of dysentery [*h*] does not accord with the history of the disease as I read it. The result of my study of this malady is that it is the least contagious of this class of maladies—that is regarding contagion as a primary emanation direct from the body, [as in small-pox] and not as a secondary result of desiccation or other change in the discharges from the body [as in cholera]. It seems to me that it is extremely doubtful whether it is contagious at all in the sense indicated, as I have already stated. And certainly the instances given by Trousseau, by way of showing its contagiousness, are by no means conclusive, or final, as to the point at issue between Stoll and himself. Whether dysentery be contagious or not, the cases cited leave the question almost untouched. The fact that a regiment in Algeria, marching from station to station with dysentery in its ranks, communicated the disease to every station where it halted, certainly looks like strong *primâ facie* evidence of contagion. But yet the fact is quite compatible with what may also be a fact, namely, that the disease was not communicated by contagion, in its usually recognised sense.

375. And here I must break off to remark upon the extreme poverty of the language employed in these questions. There is such a want of precision in the two words in use, that it is next to impossible to determine what a writer really means to convey by "contagion" or "infection," and quite impossible to state one's own views clearly and intelligibly by them without having to resort to cumbrous paraphrase. The noun "*contagion*" does duty, for instance, for the spread of disease by *fomites* and for its communication by actual contact; while "*infection*," which I take to have the larger signification, includes every form of causation and propagation, by air, water, discharges, and so forth. To crown the confusion arising out of the necessity for selecting one or other of these words, some authors use them, either in the converse sense to that commonly understood, or indiscriminately, as though they had a precisely analogous meaning, or were convertible terms. The man who should invent a code of scientific signals in this matter, by hoisting which the exact inflection of thought could be displayed, would relieve the subject from a deal of its present vagueness and

uncertainty. As it is, I can only assume that Trousseau speaks of the contagiousness of dysentery in the same sense as the contagiousness of scarlatina, by the context. He says both diseases are contagious, but he does not say they are both contagious in a similar way. And, indeed, I could hardly imagine at first, even though he says dysentery is "exceedingly contagious," that he meant it to be inferred that dysentery is exceedingly *catching*, as scarlatina is exceedingly *catching*. There surely must be some error here—some slip of the translator, or some nodding of the author. But no—this cannot be supposed either. For in Vol. III. Trousseau has a Lecture on Contagion; and in that Lecture I find he puts his view as to the contagiousness of dysentery, in a manner which can leave no doubt whatever as to his meaning. He says at page 31:—"Thus you see that typhus which was originally caused "by infection, ultimately becomes quite as contagious as small-pox. "The same statement is true in respect of dysentery and other "epidemic diseases."

379. Here then it is deliberately stated that dysentery caused by infection is transmitted by contagion. I must say the opinions advanced in this matter by this author appear to be characterised by dash and boldness rather than by accurate observation, careful analysis, or close reasoning. No other writer that I know of has suggested that typhus "ultimately becomes *quite as* contagious as "small-pox." Not to mince matters, and notwithstanding the eminence of the Clinical Professor, I call this a loose statement, or an assertion not borne out by the observations of others. The history of the two diseases certainly does not warrant the conclusion that the one is quite as contagious as the other. The subject however has no direct relation to the present question, though it may reflect slightly on it. It may serve to test the degree of attention paid by Trousseau to this special branch of enquiry, and to show the value to be attached to his views as to the contagiousness of dysentery—which he has certainly not substantiated.

380. For if dysentery is so exceedingly contagious how is it that we hear of *isolated* cases of the disease in Paris? (*a*) How was its propagation stopped—by pratique? or by what other means? Were the dysentery patients in Trousseau's hospital cut off from the other patients? If not, did the disease spread at all? Nothing of the kind is recorded as having occurred at the *Hôtel-Dieu* and no further mention is made, throughout the Lecture on Dysentery, of contagion. That patients were treated by Trousseau in the *Hôtel-Dieu* is shown by the following:—"You have had an opportunity of studying the "disease in the clinical wards; and during the last few days, you "have seen in bed 5 of St. Agnes's ward, a man, and in bed 11 "of St. Bernard's ward, a woman, suffering from dysentery. The "man is convalescent. The woman died: and I showed you the "terrible intestinal lesions which were found on examining her "body." At page 176 occurs:—"Having seen at Tours, Versailles, "and Paris, several epidemics of dysentery, which carried off men "in the prime of age and strength, as well as old people and young "children, I am able to speak, and I wish to speak, from my own

"personal experience." Though Trousseau is now upon the point of treatment, it yet seems strange that, with all his experience, he should not have adduced one clear case of the exceedingly contagious nature of the disease, that came under his own personal observation; but should have fallen back on Algiers to support and illustrate the position he has taken up. Surely so exceedingly contagious a disease did not involve the necessity for travelling out of France, where it is so common, in order to show his pupils the force of his remarks. And yet the inference is that this necessity existed:—especially since we find that with all Trousseau's theoretical belief as to contagion, he practically ignores it in the *Hôtel Dieu.* In his Lecture on Contagion [p. 57] I find the following:—"In the ward "of an hospital, containing patients both with scarlatina and small-"pox, other patients, occupying beds far removed from the latter, "have taken scarlatina." Has anything analogous to this been observed with reference to dysentery? I doubt it extremely. And even where infection has taken place in a ward of an hospital, I suspect a rigid and searching examination into all the surrounding circumstances, would show that the disease had not spread by *contagium vivum*, by direct, primary, fomitic, exhalations, or emanations of particles of organic matter from the body of an infected patient; but by means of the dejections of the person infected. That is to say, so as to leave no possible doubt as to my meaning, I believe that in an hospital ward of 50 beds, half of which are occupied by dysentery patients and the other half by general patients, not one of the latter could contract the disease, if only the dysenteric discharges from the others were all removed to such a distance that the atmosphere of the ward could not possibly be affected by them subsequently. Of course if these discharges are retained in the ward for any length of time, on the floor or about the bedding, there will be danger of infection. [Not from the fluid, or semi-fluid, contents of pans, buckets, or receiving vessels, be it understood. I hold such contents to be innocuous unless when swallowed, or introduced by absorption. But the danger lies in the discharges becoming dry and pulverulent; in which case the water-plant of dysentery, as I conceive, may find admission into the air.]

381. Indeed it appears to me that dysentery is no more contagious than ague is contagious. It has not been suggested to my knowledge that ague evolves any contagious principle; yet I strongly suspect that if, in the supposititious case submitted, ague patients were substituted for dysentery patients, the converse result might be brought out by reversing the terms of the proposition. Let the excreta of the intermittent patients remain in the ward, under conditions to become dry, and to be launched into the air on "rafts," as Professor Tyndall says. Is it a very wild notion that in that case the water-representatives of the *Palmella*, when diffused in the air, would be efficient agents of infection? For my part I opine that the chief reason why ague has not been pronounced to be contagious hitherto is that patients are very rarely confined to bed with it. And this reminds me that no one ever dreams of associating chronic dysentery with contagion. What specific difference is there between walking

about and lying down which should create a specific contagious morbid product in the one form of this disease? But chronic, or acute, I have not yet met with a single recorded instance of the so-called spread by contagion, that is not perfectly explicable without the assumption of contagion: while there are numberless negative cases in which, on the "exceedingly contagious" principle, dysentery should spread occasionally from *isolated* cases, but in which it stops short without any attempt being made to arrest it.

382. There is no occasion to enter at length upon the causes which led to the infected expeditionary columns of the Algerian army leaving a legacy of dysentery at all the military stations where they halted. To prove contagion, the possible spread by the dried germs of the disease in the evacuations must first be eliminated and shown to have been impossible in these cases. Then too the degree of surface-pollution at the stations must be taken into account. The internal state of the latrines at these places may have been such as to induce the troops to go outside the encampments rather than resort to them; in which event there might have been a wide substratum field, ready to receive the sporules of the mildew which, I assume, would form rapidly on dysenteric stools when left upon the surface of the ground.

383. The same remarks apply in principle to the epidemic caused at Marseilles. [*h*] Some observations have already been offered on this epidemic [146], and there is but little more to add. It may, though, be pointed out that notwithstanding 2432 men were taken into the hospitals of Plymouth, all of whom had either typhus or dysentery, not only did the latter disease not spread in the town, but there is no mention of anything in the shape of contagion among the attendants or others at the hospitals. One medical man caught typhus, but no one seems to have taken dysentery. I submit that these two Algerian instances are insufficient to establish Trousseau's position that dysentery is "exceedingly contagious;" and that much more copious, ingenious, and cogent argument must be employed before it can be established.

384. The severe epidemic which ravaged Paris in 1859 was simultaneous, or nearly so, with a severe epidemic in Norway. The historians of this latter, Messrs. C. Homan and C. Hartwig, hold to the same belief with Professor Trousseau, and more decidedly. They not only concur in ascribing the propagation of the flux throughout the Krageroö District to contagion, by means of volatile particles given off from the human body; but they arrive at the singular conclusion that a healthy, uninfected, person, may convey the contagious principle and may infect another person without suffering from the flux himself! After what has been said, I do not feel called upon to reopen the lists;—the more especially since Messrs. Homan and Hartwig prove abundant surface contamination and make out a fair case of water-pollution. I have referred to them chiefly to show that Trousseau does not stand alone, but that there is a school of pronounced and very advanced contagionists.

385. The question of contagion, or non-contagion, in dysentery, is a very important one to determine; though I do not know that its

settlement one way or the other would materially affect the theory I propound as to the causation of the disease. It would not upset the main proposition that the source of the specific poison is in human excrement, even if it were shown conclusively that dysentery is exceedingly contagious. At the same time it would tend greatly to modify, if not to break down, the subsidiary mildew hypothesis. Therefore I have given this part of the subject considerable attention; for, although the mildew hypothesis is subordinate to the theory of the causation of dysentery by excrement, it is a far more important thing to verify the hypothesis than the theory. In taking leave of Trousseau, I submit that there is nothing in his Lecture that militates against, or is irreconcilable with the theory of the causation of dysentery by human excrement; whilst it contributes some negative testimony in support of it.

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386. Professor Niemeyer's observations upon the etiology of dysentery appear to me to lack but one thing to have made them perfect—the substratum of the "low vegetable organism." There is not a sentence quoted which does not bear the impress of careful observation or sound induction. His hypothesis that the "dysen-"tery germ" is a "low vegetable organism," I have ventured to supplement by the hypothesis that this "low vegetable organism" is some form of mildew on excrement; and I must add that whenever I have had grave doubts in my own mind, as to the soundness of arguments into which I have been led on the strength of these low vegetable organisms, I have been fortified and reassured, on studying over again the acute, profound, and sagacious deliverances of this philosophical mind. When I have been almost startled at the consequences of the large conclusions I have arrived at, on the assumed premiss of a fungoid germ of fœcal origin, I have turned to Niemeyer and I have found support.

When I have reflected, indeed, upon the astuteness of the reasoning displayed in arriving at the deduction that the specific germs of these diseases are of vegetable origin, I have marvelled that the processes of induction should not have gone one step farther and led to the source of these vegetable germs. It now seems incomprehensible to me how their substratum should have been missed;—not in dysentery, perhaps, for its causation by surface excrement has not been suspected; but the intimate and almost direct connection of enteric fever with excrement, has long been known. All modern authors point to fœcal matter as one of the chief factors of typhoid. Since, therefore, Niemeyer and others have for some time past arrived at the conclusion that a low vegetable organism must be the infective agent of typhoid, it does seem strange that the thought of a mildew on excrement should not have occurred.

387. I would draw the reader's attention to the cautiousness with which the question of contagion is touched; and to the contrast on this point between the steady dry light of Niemeyer and the fitful uncertain glare of Trousseau. The careful German contents himself with saying:—"for, while it has not been proved that "one person catches dysentery from another," &c. I will merely

add that every word of Niemeyer is perfectly compatible with, and indeed highly favourable to, the theory of the causation of dysentery by excrement under the conditions submitted in the Propositions.

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388. The first thing that struck my attention in Professor Maclean's Article was the phrase "pre-sanitary age" (*b*) as connected with dysentery. There would appear to be some confusion here as to period. Read by a subsequent paragraph, (*d*) the pre-sanitary age was before dysentery "ceased to be a destructive disease" in England. The ceasing of dysentery then should mark the line between sanitation and no sanitation. But the context makes the matter somewhat obscure; for Professor Maclean goes on to say that dysentery "disappeared before a higher civilisation and what "it brings in its train;" and the things enumerated evidently refer to hygienic, or farming, improvements introduced during this century: whereas dysentery had undoubtedly ceased to be epidemic long before the commencement of the year 1800. [The comparatively trifling and exceptional outbreak in Bolton, in 1831, does not touch the present question.] In fact when we come to analyse these passages, so as to arrive at the sense in which the phrases "pre-sanitary age" and "a higher civilisation" are used, and the meaning they are intended to convey, we find that Professor Maclean implies by the latter a much more advanced stage of sanitation, and a far more perfect and complex hygienic machinery, than could have obtained at the time when epidemic dysentery disappeared. As there have always been some rude notions of hygiene in England, I assume that what may be called the age of sanitation dates from somewhere about the time when public attention began to be aroused to the necessity for corporate, or municipal, or parliamentary enactments, of a special and elaborate nature, touching the general health of towns and cities. If this be the correct deduction from the premisses, and it seems to be within the spirit and intention of the words, it is a consequence that dysentery should not have ceased until after the sanitary improvements effected in this way had produced a wide-spread beneficial change. The extinction of dysentery should have been one of the results of this change. But dates interfere with this view; for the dying out of dysentery in England was long antecedent to the introduction of the measures referred to. This would be making the effect precede the cause. While I am quite prepared to admit that dysentery disappeared before a higher civilisation, I conceive that an undue importance has been assigned to underground alluvial pollution by organic matter, and an undeserved credit given to subsoil drainage and general hygienic measures, as regards ridding England of this particular malady. No doubt the present hygienic system of England is sufficient to preclude dysentery, because it so happens that the means employed not only remove the ordinary organic refuse of communities, but clear off the surface the special organic matter which, when left on the surface, originates the dysentery germ. Of course practically the

flux recedes as hygiene advances, because the exterior or superficial cleanliness of towns is as much a part of sanitary reform as drainage, sewerage, water-supply, &c. The greater includes the less; and, supposing all impurities to be swept away, the particular goes with the general. There cannot be the slightest doubt, therefore, that dysentery would have been effectually exterminated by present sanitary arrangements. But it chanced that dysentery had already disappeared from England for nearly a century before they were initiated, in the sense and on the scale implied. It is clear, therefure, that its disappearance was not a result of those arrangements; and moreover it must be patent that its disappearance took place in spite of the very urgent necessity for those arrangements. In fine whatever salutary effects the greater attention to hygiene may have produced, the disappearance of dysentery in England could hardly have been one of them. For granting that effective hygienic measures have been in active operation throughout England for the last fifty years—which is to grant a good deal—it cannot be gainsaid that epidemic dysentery had altogether disappeared for at least fifty years before that—at all events in London. To suppose that dysentery was eradicated by means of hygiene, as now understood, is to suppose a disease to have been cured by a remedy which was not discovered until after the disease had left.

389. It has been admitted that the extinction of dysentery was due to "a higher civilisation"—though exception has been taken to the inclusion of hygiene within the phrase. It may be as well therefore to show the distinction I draw between the two things, and to show how far civilisation was instrumental in rooting out this foul pest, which, according to Dr. Chevers, attacked all ranks and conditions of Englishmen, laying low some of our crowned heads and carrying off several of the mighty men of our chronicles. Though it may not be clear how far his authorities are to be relied on, Dr. Chevers names Edward I., Henry VIII., James I., Charles II., Prince Henry, son of Henry II., Dudley, Earl of Leicester; Walter, Earl of Essex; Cardinal Wolsey, and Cromwell. Some of these had remittent fever, or simple ague, or pure typhoid, I should say, with dysentery tacked on. But it is not material;—the flux was probably a powerful ally. The share which civilisation had in suppressing the disease, I conceive to have been that slow and gradual change in the habits and customs of our forefathers which was brought about in the first instance, most probably, by the increase in the size of the principal cities and the consequent encroachment on the fields and open spaces surrounding them. For there is no use in attempting to disguise, or conceal, the fact that the excrement-disposal system of the English three centuries ago was very nearly the same as that of the Hindoo—without the chatty. At the time even of the Great Plague privies and cess-pits had not been introduced into general use, as may be gathered by the sanitary regulations issued by the Lord Mayor and Aldermen of London:—"First "it is thought necessary and so ordered that every householder do "cause the street to be daily prepared before his door, and so to "keep it clean swept all the week long.

"That the sweeping and filth of houses be daily carried away by "the rakers, and that the raker shall give notice of his coming by "the blowing of a horn, as hitherto hath been done."

390. This glimpse of primitive hygiene shows that houses were not provided with privies in London at that time. The necessity for the promulgation of the regulation speaks volumes as to the state of the surface of the city. The advent of the Plague is not to be wondered at; while there must have been abundant material for the evolution of the dysentery germ ever present in the fœtid streets. When such a condition of things was tolerated in the busy part of London, it may easily be conceived what must have been the state of the fields immediately adjoining. Any one may apprehend the successive stages through which the citizens had passed from a lower civilisation up to the time of the Plague; and he may trace the gradual steps of the civilisation which ended in the final adoption of a more cleanly and decent system and the consequent refinement in the mode of life. How slow the social movement was, may be gathered from the tone and allusions of writers down to Queen Anne's period, and even later. The openness and coarseness of speech among the highest classes leave no doubt as to what must have been the general habits of the nation. Dean Swift makes it plain enough that, in his day, the material for the production of the dysentery germ was not wanting in places where its presence could hardly be supposed possible to modern notions. By the reign of George the Third, epidemic dysentery may be said to have been eradicated.

391. The higher civilisation, therefore, before which the malignant dysentery of England tardily receded, and by which it was eventually extirpated, was simply a civilisation springing out of necessity and growing by convenience and decency. The generation which saw the last of the malady had never dreamt of public hygiene, as it is now looked at, and subsoil drainage was a thing unknown. The decrease of dysentery kept pace with, and was apparently regulated by, or dependent on, latrinal accommodation and surface cleanliness. [144 and 145] The Lord Mayor's sanitary edict was probably one of the first efficient measures towards its extinction. When the system of receptacular arrangements, and the plans of superficial scavengering, were so far complete that fœcal accumulation could no longer occur in the highways and in the open spaces, to any large extent, the elements for the elimination of dysenteric poison, as I conceive, were no longer present in deleterious amount. In fine, no sooner was the foul source of contamination swept off the face of the earth, out of air and light and into darkness underground, than the last vestiges of epidemic dysentery were swept away with it—never to return, whether modern hygiene had intervened or not. England passed beyond the dysentery stage of civilisation, in which the nations of Europe are even now struggling, by accident and not by design; and the very means by which she unwittingly escaped from dysentery, have plunged her further into the slough of enteric fever—out of which I fear she will have far greater difficulty in extricating herself.

392. The "looseness" of Ireland [*c*] I claim as a singular illustration of the agency of surface fœcal matter in the causation of dysentery. For it is notorious that the Irish peasantry adhered to the early English custom down to times quite within the memory of living men. The perpetuation of the disease therefore, in that country, so long after it had been stamped out of England, is strong confirmation of the theory of surface-pollution. Its gradual decline since 1818 is fully accounted for by the more decent habits exacted by modern social notions. It certainly could not be explained on hygienic principles. The epidemiologist may learn from this sadly instructive field of enquiry how typhus, typhoid, and relapsing fever, may be substituted for, or may remain long after dysentery; and how readily cholera finds an extensive substratum in such a country. It need not excite surprise, either, if Scotland brought down epidemic dysentery to comparatively late times—though the northern climate furnished the conditions more rarely.

393. The frightful mortality of dysentery in India [*e*] is another link in the chain of evidence for the theory of causation by surface excrement. For of all countries there is not one so foully contaminated as this—not even China, though it is bad enough. There is, however, this essential difference between the pollution of the two countries, which may render that of China more deadly at certain seasons and places: namely, that the Chinese spread the material in a thin layer over the soil, for agricultural purposes; and though this artificially constructed substratum field is a most prolific source of mildews, it may be doubted whether it is so pernicious in the long run as the Hindoo objection to convert fœcal matter into manure, and the consequent continuance of the substance on the surface throughout the year. Enough has been said elsewhere [Cholera, &c.] upon the subject to show how it is that dysentery always has been, and seemingly always must be, a contingency in Indian life.

394. The views expressed by Professor Maclean touching the variety of causes that have been assigned for this specific affection [*h* and *i*] must be concurred with. They are unanswerable. I cannot, however, yield that "just in proportion as we have banished "malaria, [from within the soil] so have we got rid of dysentery." [*j*] I am neither able to perceive that there has been a proportionate diminution of dysentery regulated by the banishment of alluvial malaria, nor am I open to admit that the two things have any relation whatever. I cannot trace the dependence of one upon the other, either in England or elsewhere. On the contrary, there are instances innumerable in England where there has been of late years a vast increase in the quantity of organic matter in the soil without the poison of dysentery being generated—or without its being propagated when actually introduced. This is very easily demonstrated. At the time when Mr. Radcliffe made the reports already referred to [148] upon the condition of Leeds, Manchester, Birmingham, Liverpool, and the other large towns, it is clear there was a larger proportion of organic matter in the soil on which these places stand than there was fifty years before. It cannot be denied that

Liverpool contains a vastly larger mass of decomposing material in the ground now than formerly; or that during the last fifty years a great number of patients suffering from dysentery have found their way there. Yet neither has dysentery been generated *de novo*, nor has it extended itself by propagation in consequence of introduction. Typhoid fever and diarrhœa have been the products of the malaria in Liverpool, no doubt, and cholera would find large means of reproducing its germs there—but not dysentery. The causation of dysentery is a thing apart from alluvial malarial decomposition—the conclusion of Dr. Baly to the contrary notwithstanding.

395. The celebrated Gulstonian Lectures have been before referred to, [25] but they demand further attention. Passing over the first admirable Lecture, which relates chiefly to the lesions in the colon, the first point I note in the second is the conclusion Dr. Baly arrives at, that the dysentery of Europe is identical with that of tropical countries—a conclusion which is amply justified and clearly sustained by the argument. The connection of typhoid fever with dysentery is the next material question, though before coming to it, I remark a passage to be found on the way; namely, "amongst the "many hundreds of cases of dysentery which have occurred in the "Millbank prison during the last seven years" [i.e. from 1840 to 1847]. This amount of disease at that period is most significant as to causation—but of this presently. The relation of tubercular phthisis to dysentery, the consideration of which precedes the subject of typhoid fever, is highly interesting, but is remote from the question of origin.

396. Dr. Baly opposes the view of Rokitansky that typhoid fever is never found conjoined with dysentery. He says:—"I shall at "present only remark that at Millbank prison it has been frequent: "and that in the fatal cases the characteristic lesions of the two "diseases were many times found perfectly developed." The matter has been placed beyond a doubt I should say; and, indeed, I cannot understand how it could have been doubted by any one who had any knowledge of the history of dysentery, and had learnt how it spares no disorder. I refer to this alliance between the two diseases in the prison for the special purpose of fixing the presence of fœcal matter within its walls. The description of the symptoms of dysentery comes next; and, as I learn from the medical men of this colony, it is an exact and a vivid picture of the flux of New Holland. In this part of the Lecture I must single out these sentences, because they confirm what has been stated previously in this volume, and because they illustrate a point in connection with causation. In speaking of the diffuse gangrenous form of dysentery and the rapidity of the termination, Dr. Baly observes;—"This form of the disease has at "the Millbank prison, generally attacked the weakest subjects; but "there have been exceptions to this rule. Three strong and appa- "rently healthy men have perished from it, and the same thing has "been observed elsewhere." This fact, which has been noticed here in Australia over and over again, completely knocks away the ground from under "predisposing causes" and "the epidemic constitution." It shows they are in no way essential to the receptivity of the

poison. It establishes completely what Niemeyer lays down—that the "dysentery germ grows, flourishes and increases outside of the human "body and persons staying near its locality are in danger of being "attacked by it." Let a man be never so healthy, if he swallows, or inhales, these germs, his health is no estoppal to their producing their physical effects.

397. The peculiar nervous complication in certain years at the Millbank prison referred to by Dr. Baly, I must leave, as being outside my present purpose. I will however extract one passage which relates to causation. Dr. Baly says:—"During the months of December 1841 and January 1842 several cases of dysentery of unusual severity occurred amongst the prisoners, and in the succeeding months they became so frequent as to constitute an epidemic." This fact of the flux occurring and spreading during the coldest months of the year, adds force to the arguments adduced as to the non-propagation of the disease at Plymouth when the troops were landed there. [146, 383, &c.] In the second Lecture Dr. Baly mentions incidentally that in the New York prison of Sing Sing, "there "has been the same prevalence of dysentery and other diseases un"doubtedly produced by the agency of malaria."

398. The third Gulstonian Lecture is the most interesting and the least conclusive. The acumen, shrewdness, and penetration shown in the preceding Lectures are baffled by causation. I cannot now go through the whole Lecture, each paragraph of which affords a text for amplification. Nor is there any special need to do so; for most of the argument employed by Dr. Baly has been directly or indirectly touched upon in preceding remarks. I will merely select some of the more salient points for a few additional observations.

In 1823 the physicians of the Millbank prison "reported as their "final opinion that the disease had been produced by a local noxious "influence." This is something gained; for though the term noxious influence is vague, or, as Dr. Baly neatly puts it, "a very "general one," yet these cautious physicians have at all events committed themselves to the opinion that the noxious influence, whatever it was, was local. In the second paragraph of this Lecture Dr. Baly delivers himself in a manner which at first sight almost leads to the belief that he had arrived at the view I had taken. He says:—"Here, as in other instances where dysentery is endemic in prisons, "workhouses, or lunatic asylums, the cause really producing it is, I "believe, a malaria rising from the surface of the ground around the "building." But in the next paragraph the meaning he attaches to a "malaria rising from the surface" is definitely shown. He there says, "dysentery and bowel complaints" prevail most "when from "the state of the soil and atmosphere, the decomposition of the "organic matters in the soil is necessarily most active." And he takes the view that the malaria comes from *within* the soil, throughout. In fact when I find that human excrement is not once alluded to in the Lectures as the specific source of dysentery, either on the surface or below the surface of the ground, it tells me that Dr. Baly has not touched, though he very nearly approached, the causation of the disease.

399. The reader who shall attentively consider the reasons Dr. Baly gives for his conclusion that malaria is the efficient cause of the flux, will, I think, be struck by the singular cogency of these reasons for the theory of fœcal causation. Certainly I may say that, so far as they go, I have not been able to adduce any stronger proofs of my own propositions than are contained in these Lectures. For, curiously enough, where Dr. Baly comes on stumbling-blocks which, as it seems to me, he vainly attempts to move by ingenious, though inconclusive argument, the view of excrement pollution throws aside the difficulty at once and in the simplest way. In combatting the objection that may be taken to his conclusion, Dr. Baly first refers to the belief that diet is concerned in the causation of dysentery. He disposes of this most completely. He "visited many prisons, work-"houses, barracks, and lunatic asylums;" and he found that the disease "prevailed in barracks and lunatic asylums where the dietary "was abundant and were absent from prisons in which the allowance "of food was scanty."

400. The next assumed objection however, is not met so happily; for in his answers to it, I venture to submit Dr. Baly proves too much. The objection is "that the inhabitants of the immediate "neighbourhood do not suffer in a similar way. This is true. Even "the private families residing in the Penitentiary are seldom at all "affected with the prevalent bowel complaints, and very seldom "indeed in a very severe degree." The reply is that these free persons "are not constantly confined to the atmosphere of the "locality;" and if the reply had stopped there, it would have been a full and sufficient reply. But the subsequent reference to the fact that these persons have more creature comforts and more cheerful society is not mere surplusage; either it is beside the question, or it clashes with the principle laid down in the preceding paragraph. To say that one who inhales less malarial air than another, is less liable to malarial infection, is a clear, succinct, intelligible, proposition; but to say that the condition of mind or body influences the result, is to introduce the element of an unknown quantity and to complicate the matter by needless addition. For what proof is there that if the well-fed cheerful man inhaled the same amount of malarial air as the under-fed cheerless man, he would not take the infection as readily? Of what value is it to say that when "barracks have had unhealthy sites, the officers have "been far less affected than the private soldiers," in order to show that the comparative immunity has been from the bodily conditions of the two classes; unless at the same time it be shown that the officers and privates were similarly exposed to the malaria? All these insufficient arguments tacked on to the one sufficient one, are, moreover, opposed, by implication, to the results Dr. Baly arrived at by his visits to the different prisons and other establishments. He would seem to have overlooked this, as well as the fact of the "three strong and healthy men" being attacked. [396]

400. The question of the relative immunity of different grades of people to malarial attacks is not in a satisfactory position to my mind; and I am tempted to digress for a little to skim over the

surface of it. The vagaries or anomalies of an epidemic, as its manifestations are frequently considered, have no doubt their fixed though undetermined laws. The manner in which certain individuals are apparently singled out, either for attack or for exemption, is of course the unvarying result of differing conditions. The difficulty being to appreciate, or to seise or apprehend the conditions, this constant result has to some minds the appearance of an inconstancy, or a capriciousness, in natural laws—which is absurd. I start then with the postulate that every seeming exception to ordinary rules in epidemics, is but the natural inevitable outcome of such conditions as may be accidentally brought together. And I propose to consider how some men escape altogether and how some are affected in various degrees during an outbreak of dysentery. Of a thousand troops in an infected locality a hundred shall be infected. Of this hundred ten shall die within a month, twenty shall have a severe form of chronic dysentery lasting from six to twelve months, during which period five deaths shall occur. Of the seventy some shall have short but sharp attacks; others shall have slight but lingering symptoms; and the remainder shall have the flux with every intermediate shade of intensity. Of the hundred infected, by far the larger proportion shall be debilitated from some cause or other, but many shall be men in the prime of life and in good health. Of the 900 uninfected there shall be by far the larger number robust and well conditioned; but many shall be quite as debilitated as the most debilitated of those who shall have been attacked. The conditions as to food, clothing and labour or occupation, shall be equal. The problem is to determine how the results of the malaria have been induced. I cannot pretend to solve the problem, but I will offer a few suggestions that may assist in the solution.

The first thing to place on a definite footing is susceptibility, or receptivity. How far is the physical condition of a man concerned in the infection, or non-infection of his system by a malaria containing the germs of dysentery? The conclusion I arrive at is that the state of a man's health is a most material circumstance and may determine whether he shall, or shall not, be infected. A weakly man in camp is undoubtedly more prone to be infected with dysentery than a healthy man. So far I am in accord with all writers. But when I come to inquire why the weakly man is the more liable to infection, I find myself at variance with all authors whom I have consulted. The universal opinion would appear to be that, in these cases, the weaker man is the more often infected because he is less able to resist the malarial poison. The organism is affected by dysentery because of its internal condition; or, in other words, because of the state of the constitution of the individual. My explanation of the fact that weakly men contract dysentery in an epidemic more frequently than healthy men, is that the weakly, through physical or mechanical agency, inhale a larger proportion of the dysentery germs during the day and night, than the healthy; and that the constitutional condition of the system has no relation to the reception of the poison. That is to say, I conceive that if perfectly healthy men inhaled the same amount and kind of malaria that decidedly weakly men inhale during an epidemic of dysentery, they would be attacked in precisely the same proportion; and that the reason why they are not attacked in the same proportion, is simply because they do not receive into their organisms the same quantity and description of dysentery germs. I speak of receptivity only:—the after-course of the disease is another matter.

By this view, it may be understood how weakly men occasionally escape and the strong are sometimes infected. For though as a general rule the weakly are more likely to take dysentery than the strong, for the reason assigned, yet cases may be conceived where the very fact of the weakness has been a safeguard, and where a strong man has fallen a victim by reason of his strength. To my mind the whole question of receptivity, or susceptivity, resolves itself into one as to the amount and kind of germs mechanically introduced into the system. I will, therefore, indicate briefly some of the modes by which individuals unconsciously control the degree of their own empoisonment—by which comrades inhabiting and sleeping in the same tent submit themselves variously to the influence of malaria. I assume that malaria is an air containing infective particles of some kind; and that the particles of the dysentery poison, whatever it may be, are variable in size and potency. Gravitation will determine the degree of elevation these particles shall attain in the atmosphere; and, as a general rule, the heavier will be nearer the surface, though currents of air may disturb this condition and introduce special results. The weakly men in a malarious camp, it seems to me, expose themselves to its malaria more than the others by clinging to their own quarters and by lying about when off duty. A soldier in a weakly state throws himself down in his own tent, or he picks out a shady spot, or a place where he may bask in the sun, as the case may be. He therefore not only breathes the air of one particular locality for a greater number of hours during the twenty-four than his comrades, but he is most likely to inhale the larger and possibly more virulent particles of infective material. Hence I conceive that in a large number of troops the chances are that dysentery will attack a larger proportion of the weakly in this way. But it may happen that a sickly man may be quartered in the least malarious part of the camp; in which case his hanging about the neighbourhood of his own tent may protect him. Possibly also when his healthy comrades lift up the sides of the tent at night for fresh air, he may be so chilly as to protect his head and face from the night dews which they disregard. Thus the only seemingly likely man of them all to be affected, may be the only one to escape. So again in a tent where they are all weakly but one, that one healthy man may succumb to malaria while his weakly comrades get off. And he may bring it on himself in a variety of ways. If the tent is in a good position, he may stray from it into a malarious quarter. If in a bad one, he may expose himself recklessly at night by getting near the entrance, or by some other device to breathe the external air which the others avoid. In fact the infinity of variations and modifications in these social arrangements of camp life leads to endless, unexpected, and apparently inexplicable, results.

And besides the question of health, the peculiarity of the configuration of the body and of the mode in which its functions are fulfilled, may largely govern the receptivity of the individual. Height may affect the result: So may the presence of a heavy moustache, and an abundant growth of hair in the nostril—nature's respirators. The mode of breathing; various habits in sleeping; drunkenness [by leading to exposure]; the site of a tent; its distance from the actual source of the malaria; prevailing winds; and a host of things influence the number and nature of the dysentery germs that may find their way through malaria into the system of any given resident in a camp. The amount and effectiveness of the poison introduced into the organism may be dependent entirely upon accidental physical circumstances outside the body, or they may depend partly on external, and partly on constitutional conditions. But I believe that in no case will the strong be exempt, if they inhale an equivalent of germs with the infected weak.

On the principle thus roughly suggested and imperfectly worked out, I can understand the phenomena of camp dysentery; and on no other principle. It accounts for the fatal cases and for the mild cases, and for the exempt. It explains how men who should theoretically take the disease, go free; and how the strong and vigorous fall by the side of the untouched feeble. I think the problem proposed as to the thousand troops may be solved by an adaptation, or an extension, of this view. I do not overlook the hypothesis that a depraved or putrescent state of the blood, or a tendency to change from its normal standard, offers greater facilities for the multiplication of morbid germs. This is an old proposition; and it may or may not be sound, without affecting the question of receptivity—except in one way. If sound, it might be the means of inducing an attack of dysentery of a more severe kind from the same amount of germ material, or it might even create a bloody flux where there would otherwise have been only diarrhœa. A more or less scurvied state of an army, therefore, might govern the degree of the malignancy of an epidemic and also its extent; but the point has not yet been established, and it is by no means clear that it will be sustained. Scurvy undoubtedly fosters dysentery, by engendering listlessness and dislike to exercise; and, when far advanced, inability to move. Yet there can be no doubt that a large proportion of scorbutic men in a camp may nevertheless escape the epidemic.

401. To return to the Gulstonian Lecture. Dr. Baly says:—"It "has been asked whence comes the malaria? There is, indeed, no "marshy ground near the prison, no considerable extent of stagnant "water: there are none of the more obvious sources of miasm." He then searches for sources and, after examining the site, concludes that "the exhalations of miasms from the ground around the prison "is by no means impossible." Dr. Baly, however, admits, with that frankness which every genuine enquirer after the truth invariably displays, that there is no special miasmatic character about the prison. He says:—"But, at the same time, I must remark that the "site is not an eminently unhealthy one: this, indeed, is proved by "the fact that the inhabitants of the neighbourhood, and the families

pletely obviated, or materially lessened? [There may still be some typhus or enteric fever, for what I know—but I should suppose not.] And can the conclusion be resisted that the same processes by which one malarious site has been converted into a salubrious spot, would be followed by the same result in all similar cases? I submit that what has been done at Millbank may be done everywhere. The soil has merely been redeemed from the noxious influence of man and restored to its pristine state. And the civilised world at the present time is but a Penitentiary.

405. It was my intention to have discussed the "gaol fever" of Millbank, but I must press on. Before leaving the Gulstonian Lectures, I have to express my sense of their great value to the literature and history of dysentery. I fully recognise the fact also that even where they are defective in matters relating to causation, they are decidedly in advance of the views of the day and take the subject some steps beyond the position in which it had been fixed. The Lectures on other points are, moreover, instructive and deserving of careful study, for their originality, research, and accuracy of observation.

406. In returning to Professor Maclean, we find an immense stride in the direction of causation in twenty years. For though decomposition in the soil is still regarded as the means by which the poison of dysentery is engendered, the connection of fœcal matter with the product is clearly recognised. Whereas Dr. Baly seems not to have perceived the intimate relation between this material and the malaria emanating from the surface of the soil at Millbank; Professor Maclean distinctly includes this form of organic matter as one among the most fertile sources of the infective agent of dysentery. [*n*] In fact throughout the whole paper on Dysentery, he shows unmistakably how closely he has gone to the discovery of the secret of causation. If it had not been for the traditional notion of malaria, as a result of decomposition below the surface, it is difficult to see how Professor Maclean could have failed to light on the true explanation. Coupling Niemeyer's view of a "low vegetable organism" with Professor Maclean's views of the connection of excrement with dysentery, I submit that the mildew hypothesis acquires an amount of support that converts it into all but a certainty.

407. There is no need to delay over the dysentery at Secunderabad. [*m*] I would observe, however, that Professor Maclean either assumes that the conditions there were the same as at Millbank, or he has a knowledge of the fact independently of Dr. Baly's Lectures. The Gulstonian Lectures are scrupulously free from any indication as to its real internal condition—indeed Dr. Baly states that the sewerage was not defective—and it is only by inference I conclude there was both surface and underground pollution to what would now be considered a disgraceful extent. How Professor Maclean arrives at the actual state of the Penitentiary, is not apparent.

408. The impure water at Calcutta and Shanghai [*r*] brings me to the question of the empoisonment of water by the dysentery germ. This matter has been forestalled by the hypothesis as to the typhoid germ. By referring to what was stated [172, 3, &c.] the reader will

perceive the principle upon which I conceive pure water is turned into bad water, by the assumption of the change of the low vegetable organism of dysentery from a terrestrial mildew to an aquatic plant. I have very few details to add to those furnished as to the water-plants of typhoid and cholera. The water-plant of dysentery, I should suppose, is not so tenacious of existence as that of typhoid; for, taking the necessary conditions for the formation of the terrestrial mildew into the calculation, it is not so likely, when it has been once submerged and converted into the water-plant, to be reconverted into the mildew form on being exposed to the air by the subsidence of water. In the absence of the actual discovery of the mildew, however, it is perhaps waste of time to speculate upon its behaviour either in or out of water. The views as to the typhoid plant will fit the dysentery plant with very little alteration.

409. There is an important practical question in connection with water-pollution which may be just glanced at in passing. It is one which applies to the pollution of water by all these specific poisons. The question is how long a reservoir of water used ordinarily for drinking purposes will remain polluted by a given amount of pollution; or, in other words, how long will contaminated potable water take to clear itself after the source of contamination has been cut off. This is a complex problem, but I suggest that the law is that the sum and kind of organic matter in the water determine the period when the poison germs perish. By analogy it may be assumed that the organic material from which they originally come offer the first substratum for the germs to attach themselves to. This material being exhausted by the vegetation, the substances in the water most nearly allied to it are then probably seized upon. Thus as I take excrement to be the base of these poisons, I assume that when all the particles of excrement have been used up, the excreta of animals, or decomposing animal tissues, would certainly be selected by the water-germs in preference to decaying vegetable matters. Indeed I have great doubts whether these specific poison germs can derive nutriment from vegetable matter. They may possibly maintain a bare existence at the expense of great modification of character and properties. However, these questions may all be determined readily enough by experiment; and they are all most vital things to determine. Hygiene cannot well go on long without such points being cleared up. Exact knowledge here is essential to those who have to see to the effluent waters of large towns and to the experts who have to deal with noxious trades. Supposing one volume of water containing a certain proportion of typhoid germs be added to a thousand volumes of water free from all organic impurities, how long will the typhoid germs exist in an effective state—or in a state to induce typhoid fever, or to multiply themselves when brought into contact with their natural pabulum again? There must be a period when these germs die out and are no more dangerous as regards infection, or further growth; and that period should be known for many reasons. And again supposing a similar problem be put as to other waters;—let one volume of water, containing the same proportion of typhoid germs as in the last proposition, be added to 1000 volumes

Of course I join issue at once, and say that the enumeration should have comprehended excrement-disposal. However, let that pass for the present. *Drainage* comes first. From the facts given by Mr. De Renzy, it would seem that Dr. Baly was substantially right in saying that it was not defective in his time, seeing that the course of the typhoid fever was not governed by, or was independent of the two improvements introduced. The statement that "the drains "are well constructed; but * * * they have to be opened out constantly to be cleansed," is very important. Was the same attention constantly paid to this cleansing work before 1854, when the typhoid suddenly disappeared? On the subject of the defective drainage of water-closets, which Mr. De Renzy does not consider so dangerous as supposed, I would observe that some well authenticated cases of typhoid poisoning from obstructions have occurred.

The one mentioned here [328–9] in the Pentridge prison is a case in point. The drainage of Millbank is not quite so clear to my mind from the description, as to enable me to come to any definite conclusion as to how far it may have been concerned in augmenting the sum of typhoid germs in the prison, prior to 1854.

To pass on to the *water-supply*. This was changed on the 10th of August, 1854, from the Thames water to the well-water of Trafalgar-square, during the time of the cholera, from which the prisoners were suffering. In six days cholera ceased, and since then only three deaths from typhoid have occurred—one prisoner being infected on admission. There has been but one death from diarrhœa and dysentery since 1854. In Pentonville for 21 years there has not been a death from typhoid. The water of this prison is procured from a deep well in the chalk. In 1852 it appears that the Thames water was suspected, though Dr. Baly did not then concur in the view taken. Dr. Baly's Report for 1849 states that the "open sewers and manufactories which fill the air with impurities" rendered the prisoners liable to fever. This was an addendum to the causes given in the Gulstonian Lecture.

Mr. De Renzy says:—"Dr. Baly was evidently surprised at the "remarkable results that followed the introduction of a pure water-"supply in August, 1864. He could not believe that the sudden "cessation of cholera that followed in six days was due to the change "of water-supply, but the continued healthiness of the prison in "after years gradually shook his scepticism, though some traces of "it remain apparent to the last." Dr. Guy succeeded Dr. Baly and the prison continued healthy. In 1871 Mr. Gover, the present medical officer, reports that "the prisoners have continued to be "free from every form of disease which could call the sanitary "arrangements into question, as free as if the prison occupied the "healthiest site in the world."

These are the principal data on which Mr. De Renzy arrives at his conclusion that the Thames water was the typhoid infective agent in the Millbank prison. And it may be granted at once, that the coincidence of change of water with the sudden arrest of disease looks very like the relation of cause and effect. But, strangely enough, Mr. De Renzy has left out of the calculation just the one thing needed to check the result he obtained. The omission of fœcal matter from a discussion as to the causation of typhoid fever is an error which vitiates all deduction. Excrement has long been recognised as being inseparably connected with typhoid fever in some way; and it is not to be discarded or suppressed. In any case it must come into the question of causation at last. For even supposing the Thames water was the efficient cause of the typhoid fever at Millbank, what was the efficient cause of the poisoning of the Thames? Let it be granted that the water infected the prisoners: what infected the water? However to the question as to the fact of infection being introduced by the medium of water. Before that proposition can be accepted, it must be shown that the residents outside Millbank drank other water, during all the years zymotic diseases were raging in the prison; and that the officials living in the prison had a different water-supply from the prisoners. For, as Dr. Baly has clearly shown, the prisoners were the only sufferers in that quarter. It must be demonstrated too, I think, that every ship leaving the port of London and taking in Thames water during the 25 years preceding 1854, was equally ravaged with the Millbank prison. One exception to this would be difficult to get over, even if 99 ships' crews and passengers were proved to have suffered to the same extent as the prisoners.

Mr. De Renzy includes dysentery among the effects of Thames water. Surely he cannot have reflected on the fact that hundreds of thousands of persons were drinking that water—unfiltered—whilst the prisoners in Millbank were drinking it filtered, and yet the disease did not declare itself outside the prison. On what principle was it that dysentery died out in London whilst the people were nevertheless procuring their water supply from the Thames? And how does Mr. De Renzy account for the one fatal case of dysentery occurring after the Thames water was cut off from the prison? Did the Trafalgar-square water funish the germs? It will be quite clear from what has been said before in these pages that I do not question, in the least, the poisoning of water by disease germs. I am quite prepared to admit that Thames water may, at certain conjunctures, have contained a considerable number of cholera germs. I consider it quite possible that water drawn from the river in the vicinity of cholera and where the discharges of cholera patients have found their way into it in large quantities, may have held a sufficient number of germs to have induced cholera in some few instances. But taking into consideration the volume of water and the tidal influences, I think the chances of typhoid infection from that source must always have been very slight. Be that as it may, it is certain that the Millbank prisoners took those chances with others; and unless it can be made out that all suffered alike—or a sufficient explanation be given why they did not all suffer alike—I cannot accede to the proposition that the Thames water was the efficient cause of the typhoid, or of the flux, or even of the cholera, in the prison. Indeed I must still fall back on excrement-pollution, both of surface and soil, for the causation and propagation of these diseases. I yet believe that the rapid extinction at last was due to some improvement in latrines, privies, water-closets, drains, or receptacles; or to flushing, or cleansing. Something was done to reduce the sum of excrement on the site, or to lessen the excrement-sodden condition of the soil. As dysentery germs could have been engendered in one way only, I say confidently there must have been exposure of fœcal matter to atmospheric influences. In plain language I infer that the condition of the privies, or the surface, or some part of the grounds, was filthy and not altogether creditable to the management, or the supervision, or inspection. If I am told that the Pentonville prison was free from the dysentery which clung to the Millbank prison, I conclude that those concerned in the conduct of Pentonville knew how to deal with convicts and had some notions of cleanliness. For making due allowance for the more favorable conditions for the development of the dysentery germ at Millbank, I cannot but think that had the system of surface cleansing at Pentonville been transferred to Millbank, there would have been a different result at Millbank. Even in Australia where the tendency to dysentery from the climate is, as compared with England, at least as 3 to 1, no such outbreaks as those at Millbank have been known from the time prisons have been built.

It must not be overlooked that the same exposure of excrement which creates dysentery, is conducive to typhoid fever, as is well shown in camps. Therefore, although infection by typhoid germs may occur in a variety of ways, the surface pollution to which I assign the dysentery of Millbank no doubt had a large share in the production of its typhoid. I strongly suspect that, in addition, there was the sodden condition of the soil and, probably, the retention of fœcal matter in the drain pipes to swell the amount of infective material.

Before any definite, tangible, or sound, conclusions can be arrived at touching the highly interesting phenomena of disease in the Penitentiary, I submit it is essential to trace the history of its excreta before and after 1854. A systematic enquiry into this matter is an absolute necessity in the present aspect of the question. And I suggest that before Thames water can be received as the solution of the malaria problem of Millbank, excrement—not filth, or decomposing organic matter, or sewage, or any of the generic things which may include this substance, but excrement—must be first dealt with and eliminated from the enquiry—a work of some difficulty, I apprehend.

Sir William Gull shrewdly evades the causation of typhoid fever—as I should have expected. He is too wary to have committed himself beyond hope of redemption upon a point of this nature. He was not likely to have declared his adhesion to any of the floating theories or hypotheses of the day; and he therefore sweeps them all away by pronouncing that "there is no scientific theory"—to which I take no exception. But as there are one or two points in the Lecture connected with the subject of the causation of typhoid on which I venture to differ from Sir William Gull, I will extract a few passages, so that the reader may apprehend the drift of my observations at a glance. After stating that agriculture and drainage had almost extirpated ague in England, the Lancet says Sir William Gull "then "expressed his belief that two hundred and fifty years hence few will "die of typhoid fever, inasmuch as this disease is as preventible as "ague. Typhoid fever is, *par excelence*, to be ranked amongst "'diseases,' because it is caused by a virus—a virus of nature—which "may get into the healthy body, and increase in it and destroy it. "It is an accidental condition and not one of the ordinary processes "of nature. * * * Typhoid fever is stated to kill 17,000 a year "in England; how great, then, must be the number attacked! It "stood among the preventible diseases, and it was important, there-"fore, to know how it originates. There is no scientific theory, but "there is a good working theory on the point. The origination of "the disease is, somehow or other, connected with drainage. It has, "therefore, been called the filth fever; hence to get rid of the filth is to "get rid of the fever. It seems as if this is really so, for Millbank "Prison was infected with typhoid and dysentery, but now the water-"supply has been changed, and the drainage attended to, and these "diseases have almost entirely disappeared. No one can approach a "case of typhoid fever without paying some attention to hygiene. It "is no use tinkering with the disease if one does not try to prevent "it, and it no doubt may be prevented. The theory is that it is con-"nected with germs which get into the blood; we know nothing "of these germs—the air is full of them. There is an idea that they "are imbibed by drinking water, and that they increase and multiply "within the body. Although this has not been demonstrated, yet it "is a good working theory."

In the first place I may observe that I concur so fully with Sir William Gull that typhoid fever is a preventible disease, that I differ from him materially as to the period when it shall become as rare as ague is now. I say that if England does not break the neck of this "enemy" in ten years, she will deserve to submit to the yoke for the 250 years given by Sir William Gull.

"There is a good working theory." I grant that much has been done by the good old rule of thumb, but I must demur to calling the principle upon which that much has been effected a good working theory, until something more is known of the results of the excrement-disposal systems lately initiated in the large towns of England. However it matters not what it is called. The steps taken to prevent the pollution of water by sewage and to get rid of filth, are undoubtedly in the right direction; and in these particulars the rule of thumb, or the working theory, has done good service—but 17,000 deaths annually detract from its goodness.

The Millbank prison has attracted Sir W. Gull's attention. He attributes the decadence of typhoid and dysentery there, to a change in the water-supply and attention to drainage. I need not recapitulate the reasons I have given for viewing the improvement in these things as being altogether unconnected with the disappearance of dysentery. Nor need I repeat how far I conceive the alteration of the water-supply was instrumental in lessening typhoid; or the share drainage and sewerage had in the work. This has all been set down—I fear with tedious prolixity. I have only to add that I must adhere to my views until they have been disproved, notwithstanding that the opinions expressed by Sir William Gull tell, by weight of authority, against them. Whether the propositions I have submitted, however, be shown to be sound or unsound, one thing is certain. There must be a good scientific theory before there can be a really good working one. Hygiene in the dark must trip. I sincerely trust, therefore, that Sir William Gull will soon be able to say that the learned microscopists of Europe have placed the whole matter on a very different footing. It is to them that the final appeal must be made to determine whether a particular theory or hypothesis is sound or otherwise. The germ must be got, its substratum made out, its conditions of evolution discovered, and its connection with the disease demonstrated; and these things are all dependent on, and waiting for the microscopist. In the mean time we can only speculate upon the origin of typhoid fever and offer our crude suggestions to the lens-worker. He is nearer to the germ than Sir William Gull seems to suspect. He is merely substratum hunting; and it would not surprise me in the least to learn that he has already got upon the trail, or has, perhaps, even before this, run into his quarry, and produced a good, if not a perfect scientific theory. I predict that Sir William Gull will soon know something of these germs and will live to see typhoid fever in England, if not as rare as ague, diminished at least three fourths.

ON

DISINFECTION AND DEINFECTION

IN RELATION TO

DYSENTERY.

414. Deinfection as against dysentery is the simplest of all sanitary work. It is but to obey the Mosaic ordinance—to cover that which cometh from the body—with a little earth—and the thing is done. Of no proposition, having induction for its base, do I feel more assured of the soundness than of this one. As a corollary, I submit that if it were possible for the whole world to follow the law of Moses from to-morrow, dysentery would be stamped out for ever. Although the flux has been supposed to be more fatal to mankind than any one disease, yet no other zymotic disease is to be eradicated with so little trouble, or by such easy means.

415. The law of Moses, however, cannot be carried out to the letter. The command could not be implicitly obeyed by the Jews themselves when they lived in Jerusalem. It remains, therefore, so to adapt it to the mode of life in cities and settled habitations, that it shall be as efficacious towards preventing dysentery and other pestilences, as it was in the camps in the wilderness. The spirit and intention of this remarkable hygienic provision must be seized and bent to the demands of civilization. And there are no inherent difficulties in the way—no insurmountable obstacles—nothing but the habits, forms, beliefs, or prejudices, of nations and individuals to overcome. The proposition being admitted and clearly established, it is merely a matter of time how long it will be before countries embrace the opportunity of shaking off their plagues themselves, or are coerced into sanitary measures by others.

416. In the matter of the disinfection and deinfection of the zymotic diseases, I suggested the proposition that no true or scientific hygiene can obtain before the infective principle of the disease has been ascertained. I therefore submitted certain suggestions, provisionally upon the strength of the hypotheses as to causation being substantiated. If the hypotheses are unsound, the disinfection and deinfection proposed are inapplicable, or will have to be modified to meet the requirements of the actual causation, whatever it may be. The same rule holds good with dysentery. No thoroughly effective, or universal, system of prevention can be carried out, before the dysentery germ has been discovered and all the conditions of its evolution in different countries determined. This is a canon in hygiene.

417. It was a part of my original design to have summed up here the arguments for the theory I have submitted as to the causation of dysentery, so as to have presented them to the reader in a connected form. I find that although the direct arguments might be compressed within a reasonable space, yet the indirect arguments, derived by analogy from other diseases and reflected in the hypotheses as to their causation, would involve writing a great part of the work over again. I must leave the matter as it stands, therefore, and let the reader form his own conclusions. If the materials, loosely strung together as they are, are not sufficient to carry conviction to a logical mind as to the connection between dysentery and excrement, all re-arrangement would be futile. The theory must take its chance as it is, and must stand, or fall, on its soundness, or unsoundness. If sound, mere defect in the manner of presenting it will not be fatal to its ultimate acceptation. If unsound, the most elaborate disposition of the matter will not save it from final refutation.

418. The hygienic canon as to deinfection for dysentery is not to be disputed. It is indisputable. Yet dysentery stands upon a somewhat different footing from the other zymotic diseases. Although the infective agent has not been demonstrated, yet the source of that agent has been demonstrated, and the principal, or essential, conditions under which the agent leaves its source have been demonstrated. At least I venture to think that it has been shown that fœcal matter is the source of the dysentery germ; and that the principal conditions of its evolution and emanation from its source are given in the Propositions. At any rate I assume the fact now. I borrow here the premiss that dysentery is caused by surface excrement, and by it only. I have no doubt upon the point, but still the theory lacks the absolute proof.

419. I take deinfection first, as being the more important branch of sanitation. The mere covering up of excrement with earth is perfectly efficient deinfection as regards dysentery. Nothing more is required. But this primitive plan is neither practicable, nor demanded of necessity in cities :—as has been demonstrated in London and throughout England, Scotland, and Ireland, as well as in Melbourne and in all well ordered cities under British rule, where the bulk of the population is of British origin. It is quite clear therefore, that covering fœcal matter with soil at the time of voiding it is not the only means by which it may be deinfected. I deduce from all the premisses before me, that simply confining the excreta, as voided, in a receptacle is an effective mode of preventing dysentery. Even the common midden and privy of England have, practically, been found equal to suppressing the flux in that country, even though in many of the large towns they have been so defective as to cause extensive soddening of the ground with excrement. Therefore I conceive that surface cleanliness of the earth is the sole thing absolutely necessary to extinguish dysentery.

420. But, as has been seen, surface cleanliness alone is liable to lead nations into another danger. Though underground pollution is totally unconnected with dysentery, it yet causes 17,000 deaths

annually in England from typhoid fever—to say nothing of those from the periodical visitations of cholera, from the ever-present infantile diarrhœa, and possibly from the ravages of diphtheria and other zymotic diseases. A thorough excrement-disposal system must have regard to this evil. I will now point out what is essential to the preservation of camps. And I will first introduce the earliest mode of deinfecting camps on record.

421. Reference has been made on more than one occasion to Moses, and although I am uncertain how far such evidence as is to be derived from the Five Books is admissible in a matter of this kind, inasmuch as it may be held to be void for uncertainty, yet I shall bring it forward for the reason that it reflects strong light on the causation of dysentery and camp diseases generally and on deinfection. The evidence moreover is of that kind which carries conviction with it. It involves no matters of religious belief and is dissociated from doctrine. It is, in fact, merely historical and relates solely to the subject of camp regulations. I do not see therefore why it may not be accepted as evidence in hygiene.

The Jewish nation was watched over during one of its most important epochs, by a leader who carefully and zealously guarded it against all those physical evils which might affect it as a race. Moses was not delicately tender, or affectingly humane, about persons, or small sections of the people. He discarded feeling and ruthlessly consigned a leper to his fate; and sternly annulled the prerogatives or privileges of those members of a favoured tribe who were halt, or blind, or otherwise maimed or disfigured, either at birth, or by accident. This percentage of the Levites he dealt with summarily. A bright white spot in the flesh, or the skin, or the "scall," duly examined and pronounced to be leprosy, entailed immediate banishment. There was a terrible simplicity in the decree—an awful severity in the doom. "Both male and female shall ye put out, "without the camp shall ye put them; that they defile not their "camps, in the midst whereof I dwell. And the children of Israel "did so, and put them without the camp." It makes a degenerate modern's flesh creep a little perhaps to reflect on the fate of these outcasts; for that they were left to die if they could not follow when the camps were struck, may be gathered from the exception made in Miriam's case. How if we were to put our Chinese lepers without our camps—though theirs is said not to be the biblical leprosy [?]

But it must not be forgotten that the position of affairs left apparently no alternative. The Israelites could not stop in the wilderness; and to construct lazar-houses, and furnish them with supplies and attendants was impracticable. And to retain these infected people among them would have been extremely dangerous, if not death to all possibly. So there was nothing left but to sacrifice them. And under the circumstances Exeter Hall itself would have sacrificed them. Moses had to conserve his people; and the general safety and well-being of the whole was the paramount thought ever present to his mind. Though he did not hesitate as to the means, his end was to keep up the numerical strength and the warlike effectiveness of the children of Israel as a nation. This object he never lost sight of, but followed it up unceasingly. He framed the most elaborate and admirable sanitary laws, and entered into the minutest hygienic details in his ordinances.

The statute contained in Deuteronomy [chap, xxiii., v. 12] is a remarkable instance of the infinite pains Moses took to ensure the salubrity of the camps. It shows that where the health of the people was concerned, he did not consider it derogatory to his high office to stoop to devise any kind of measures to prevent an outbreak of disease. And that this was the real intent and purport of the ordinance there can be no reasonable doubt. For Moses would hardly have thought it a matter for a written law, unless the importance of the due observance of the edict had been impressed upon him in some way. The subject is one which might apparently have been left with more propriety to the unwritten law; and so most probably it would have been, but that Moses felt that it required some higher sanction and authority to ensure its being religiously carried out than a mere camp regulation. He knew his people and knew they would be very likely to neglect, evade, or make light of, an ordinary rule of this nature in common life, if merely ordered by his officers, without any express formula, or divine command. So he invested this natural corporeal function with the safeguard of a solemn prescription; and, in order to impress it the more on the minds of the people, he assigned an awful purpose as a reason for obeying the injunction; and even designed a practical means by which the ordinance could be observed.

All this trouble about a matter of camp routine which at first sight looks as though it might have been left to subordinates, there is good reason to believe was aimed at the prevention of the pestilence of dysentery and other camp diseases. Knowing what we know now, the conditions surrounding the people of Israel in their journey must have been precisely those which, without the peculiar provision in question, would have been causative of the zymotic poisons that produce the flux and typhoid fever. Any person who has been present at a "rush," will be able to form some notion of the difficulty of providing adequately for the many social wants of twenty or fifty times the numbers, when encamped in tents in the bush of Syria. Bearing in mind the specific origin of dysentery, he will also appreciate the urgency and cogency of the necessity for laying down some stringent law upon the subject; and he will recognise the wisdom of him who, discarding the inefficient provisions of penal enactments, took the surest method of coercing the "stiff-necked" race he had to deal with, by not only including the command in a code of moral laws, to disobey which was to bring down on themselves a terrible curse, but by also hedging it round with an additional precaution even to insure obedience. This was done by giving to the ordinance a sacred import and a holy aim and object which, he knew, the most rebellious Israelite of them all would not dare to question, but would be afraid of in his heart. It may seem a mighty piece of machinery to have employed for so small a purpose: and to the unreflecting the means may appear to have been singularly disproportionate to the end. To some the interference in such a matter by a ruler may look unworthy, or peddling, or perhaps vexatious. But Moses was not one to fritter away either his time in framing laws about trifles, or his power by exciting contempt or needless irritation. There were elders and officers to attend to all the routine work of the camp, and to them would have been delegated this particular business, no doubt, but for the fear the great leader had lest they should not comprehend the immense importance of this hygienic law, or lest they could not enforce any measures they might devise, without supernatural aid. Few of that vast host, probably, appreciated the interdependence between "uncleanness" and disease; and fewer still would have been able to trace the close connection between the habitual neglect of a sanitary arrangement and the outbreak of a malady like dysentery. Men do not see the relation between the two things to this day; and they were not more likely to have had their eyes opened then. But Moses had evidently traced the evil to its source. He saw the effect and fitted it with the cause. To provide the remedy was the next step. By some means he became aware that the soil under his feet was nature's deodoriser—the world's disinfectant. He utilised his knowledge practically for the good of his people, but clothed its application with dread mystery to ensure its more perfect success. He introduced an available universal earth-closet and coupled its introduction with a reason for its constant use which all must obey. And he saved Israel from many a sore plague of dysentery and typhoid.

It is perhaps worth noting that there is nothing in the books of Moses to show that at any of the encampments in the wilderness the people were attacked with dysentery: for no mention is made of a bloody flux occurring during the forty years of the Exodus. Nor is this disease one of those specially named, in the curse pronounced upon those who may disobey the commandments, in the 28th chapter of Deuteronomy. By referring to verses 21, 22, 27, 28, 29, 35, 59, 60, and 61, the reader will find a terrible array of maladies set forth as the lot of those who shall infringe the statutes. But none of these diseases, curiously enough, appear to have been dysentery or typhoid, or to have had symptoms in common. The "pestilence" referred to was probably a distinct disease. The "great plagues" were epidemics, no doubt, and dysentery may be included within this generic term. But if not, the 61st verse is comprehensive enough not only to take in dysentery but all other maladies. "Also every sickness, and every plague, which is not "written in the book of this law." The 60th verse contains what may have more immediate reference.

"Moreover he will bring upon thee all the diseases of Egypt, which "thou wast afraid of; and they shall cleave unto thee."

An attentive consideration of the whole subject, aided by the light of modern knowledge, will, it is submitted, bring out these results:—I. That the Israelites were not afflicted with dysentery during their journey—unless perhaps within the first few years after the flight from Egypt. II. That their mode of living would have been highly conducive to the production of the disease under ordinary circumstances. III. That by an extraordinary sanitary law Moses averted epidemics.

[I.] In support of this proposition there is in the first place the fact that dysentery is not mentioned anywhere in the Pentateuch. Nor are there any allusions to such of its prominent symptoms as would indicate the disease. This would have been a very singular, unaccountable, and seemingly purposeless, omission, had so marked and fatal a disorder prevailed to any extent during the thirty years preceding the arrival at the river Jordan. As wilful suppression is highly improbable, and as carelessness in narrative where important things were concerned can hardly be attributed to Moses, this absence of all reference to dysentery is strong presumptive evidence against the presence of the disorder. And if the object of the curse be considered, the exclusion of dysentery from that denunciation is as conclusive as negative proof can be, that the Israelites had long ceased to have the fear of that plague before their eyes. The intent of the curse was to deter the people from the sin of disobedience, by holding out a threat of vengeance against the disobedient. Their punishment was to comprise every worldly disaster and every bodily infirmity that they could imagine, or recognise from description. And a more graphic description of earthly misery—a finer piece of Eastern word-painting designed to strike terror into the soul—can hardly be conceived. Moses did not confine himself to vague generalities when dealing with prospective disease. He first arrested and fixed the common mind by naming specially those fatal, painful, or loathsome, disorders with which they were no doubt familiar, and then ended with every known and unknown disease. His purpose in specifying certain diseases and associating with them something characteristic, was clearly to give vividness, reality, intensity, to make the picture more life-like, to burn the dread into their hearts, to sheet the lesson home to their understandings.

And if dysentery had been among them, so that the people had been accustomed to its presence, is it likely that Moses would have abstained from adding it to the diseases alluded to in the curse? Would he have failed to avail himself of this adjunct to terrorism? If the masses in the plains of Moab had seen the victims of dysentery, to-day writhing in their strong agony, and to-morrow fainting to death from loss of blood, would not Moses have enlisted such a means of furnishing an additional incentive to obey the law and the commandments? For in the whole range and catalogue of painful disorders, there is none more dreadful or horrible to bear, and none more distressing or appalling to witness. Where the intensest human suffering possible to conceive, is seen allied with, and laid on by the plain hand of, death, there can be nothing more sublimely moving to behold. And nerve matter is probably incapable of conveying to the brain the impression of greater torture than that of the feeling of the twisting of burning entrails into hard knots, or the sensation of boiling oil, or molten metal, forcing its way through the body. Indeed, as an exhibition of high mortal agony, the tormina of dysentery may take rank with the cramps of cholera, the convulsions of lock-jaw, or the spasms of hydrophobia.

No adequate reason offers itself to the mind for supposing that Moses would have omitted, or declined, to pourtray a disease of this nature and to hold it up before them to quicken their fear of breaking the law, if the Israelites had been subject to the disorder within the memory of the young or the middle-aged. It is not unlikely, however, that the camps may have been attacked with periodical visitations of dysentery during the first few years after the flight. This may well have occurred, and yet all lively recollection of the epidemic had faded away in thirty or five and thirty years. And as traditional accounts even of a plague are weak in their effect upon the feelings, as compared with a present living malady of less serious character; so Moses elected to work upon the feelings of his people with the scab and the itch, which they had seen, rather than with the flux, of which the most of them had only heard. The one was so near that they felt they might contract it any moment: the other was so remote that it might not have moved their fears, or stopped a venial sin.

The supposition that dysentery did actually occur in the journey at first, is borne out to a certain extent by the words that refer to the "diseases of Egypt," of which the people had been afraid, and which diseases they are told shall "cleave unto" them. The verse above quoted would seem to bear the interpretation that certain deadly diseases of which they were greatly afraid at the time, had apparently pursued them and come upon them in an epidemic form; and that in place of their disappearance and reappearance at uncertain intervals, they should be for ever present among, or should "cleave unto," the people. This reading of the passage will not appear strained when it is remembered that of all diseases dysentery is perhaps the most likely to accompany large bodies of men moving from place to place, but halting here and there for weeks or months; and certainly there is no disease the Israelites could well have been more thoroughly "afraid of." Typhoid fever and even the bubonic plague were most probably present when surface-pollution existed.

[II.] That the kind of life led by the Israelites on their journey was highly conducive to all camp diseases in such a climate; and that they would have been engendered among Orientals—and Europeans too—under ordinary circumstances, is a proposition which at this stage need not detain us long. If the people congregated in the camps had followed the customs of the Hindoo, or the Italian, or the Australian digger at a "rush," they would have paid the same penalty without doubt.

[III.] Assuming that the ordinance of the paddle was implicitly obeyed and universally carried out by the people; the question then arises whether it would have been an efficient mode of deinfecting the excreta of the camp. Under the conditions of that hot and arid country the simple provision designed by Moses was as perfect as possible. It was as great a safeguard to his camp as could have been devised under the circumstances, a most marvellous effort in deinfection—always supposing the ordinance could be enforced, or that it was certain to be obeyed. To ensure this, however, Moses took the extraordinary means of giving to a natural function a supernatural consequence.

It would, indeed, seem probable that the measure initiated by Moses was arrived at for the one sole object of preventing fœcal plagues. Either it was done designedly for this express purpose, or the extreme step was taken in pure ignorance of its beneficial results, and merely for the ostensible reason assigned to the Israelites. Of the two suppositions it appears most likely that the law-giver had more in view than simply preserving the cleanliness of the precincts of the camp. Not to enter into questions as to his divine mission, or authority, or direct inspiration, it must be evident that Moses was at all events a natural philosopher in advance of his age; and that he was shrewd, far seeing and enlightened beyond his fellows. He displayed rare talents as a leader of men; and contrived to keep together and control an unruly nation under circumstances that demanded greater circumspection, more varied knowledge, and a larger intellect, than has been required of man before or since. The magnitude and complexity of his operations not only exacted unceasing vigilance, but could only have been undertaken and successfully carried out, by one of the very highest order of minds;—one that combined large capacity for reason, great quickness of apprehension, and a marvellous aptitude for observation. That Moses's mind possessed these attributes in perfection there is sufficient testimony. There is nothing so improbable, therefore, in the supposition that such a man, having observed that whenever the air surrounding the camp was polluted more than ordinarily, either from longer residence than usual in one spot, or from some local, seasonal or atmospherical, cause, there was at the same time an epidemic of dysentery or typhoid raging among the people, should come to associate the two things as cause and effect. It need not be assumed that Moses must necessarily have had a knowledge of the precise connection between excrement and the specific poisons, to have enabled him to arrive at the conclusion that diseases were sometimes and in some way caused by fœcal matter. For it does not require advanced scientific knowledge to observe that noisome fœtid effluvia precede epidemics and attend on them closely and constantly; and then to connect the regularity with which these things recur and to trace a relation between them, or a dependence one upon the other; or, in fine, to deduce that they represent a cause and its effect. It did not require the assumption of a fungoid hypothesis, or any other hypothesis, to apprehend the significance of the fact that when the camp was unclean, epidemics were rife; that when a move was made and a fresh site was chosen, the epidemics disappeared; and that they were absent until the camp was befouled again, when they returned in all their virulence as before. What it does require is acuteness of perception, or exceptional watchfulness, or profound reflection, as to the natural laws surrounding us. One or perhaps all of these qualities were quickened in Moses by the responsibility of his position; and by the absolute necessity he was under to preserve the nation from reduction in strength and warlike effectiveness. It is no such violent assumption therefore that he hit off the idea as to the real cause of dysentery; and that he also improvised an efficient deinfecting measure in the simple paddle. When we find the Australian savage doing precisely the same thing with his yam-stick that Moses commanded the Israelite to do with the paddle upon his weapon, going forth abroad from his camp and covering "that which "cometh from" him, for sanitary purposes, as there is fair reason for assuming; [312]—when we find this, there need be little marvel that one who was deeply versed in all the learning of the Egyptians and had afterwards graduated as a bushman during his long exile in the Arabian desert, [where he was a shepherd of Jethro's (?)] should have conceived the design of conserving the health of his people by this primitive, but efficacious, expedient.

Curiously enough the commentators are unusually reticent upon v. 12, 13, 14, of c. 23, of Deuteronomy. I was under the impression that Mahomet had incorporated this camp regulation of Moses in the code for the faithful; and it occurred to me that possibly some light might be thrown on causation and hygiene, by comparing the parallel ordinances and by procuring the assistance of the comments of the theologians and others who might have examined the passages. As I was too much occupied otherwise to spend much time over the matter, a learned friend undertook to look up whatever might be forthcoming. The result of his search shows a singular want of interest, or lack of information, or reluctance to enter upon such a subject. In the first place it appears that the only allusion to this bodily act to be found in Sale's translation of the Koran is in the 5th chapter, and there it seems to me Sale must have made some error in rendering the words into English. The ordinance is of no importance to the present question and is in no way analogous to the paddle system; but surely Mahomet could not have said—"If any of you cometh from the *privy*." This word must have been taken, I suppose, as the nearest approach to the meaning in the text.

Most commentators, I learn, pass over the passages in Deuteronomy without any remark; but Patrick says on the words "cover that which cometh" &c.—"This is still practised by the Caribbeans, and Busbequius observes that the Turks use the same cleanliness in their "camps, making a hole with a piece of iron, wherein they bury their "excrements."

Dr. Hussey remarks on v. 12, 13:—"The sanatory regulation for "war [?] here made was the custom of the Turkish army so late as "the Sixteenth Century." [No authority quoted.] It would seem then that Mahomet—unfortunately for his followers—failed to copy the great original in this particular.

The question as to the women and children in the camps of the Israelites, I assume, was settled in a somewhat similar way to that in Canvas Town and Melbourne. Their excreta were probably removed at night; but unlike those of the women and children of this place they were probably conveyed to a place set apart and dug into the earth—the same principle being followed out, though in a different fashion, for the same reason. The law of Moses would have been futile if the women and children were not included in some way.

There is another thing to be observed—that the Jews carried out the spirit of the law even after they got to Jerusalem, where they could not well use the paddle. They evidently set apart a place within the walls of the city and had a complete excrement-removal system; as may be gathered from the reference to the "dung-gate" when they rebuilt the walls of Jerusalem. I think Moses may therefore be fairly considered the earliest, greatest, and most successful of sanitary reformers. He should have the first place in the Temple of Hygeia. He had his reward in seeing his people the finest, strongest, and most warlike of all nations. No wonder the pest-ridden races who opposed these wholesome-living warriors went down before them. Malaria is the secret of the physique of many a nation.

422. Does anybody know anything definite about the Caribbeans and their customs? If so, it would be extremely interesting to hear whether there is any actual foundation for the statement that they have observed the sanitary law of Moses, and to learn the result. The contrast between the islands and the main land of America, as regards malaria, should be most marked—if the excrement-disposal system said to be in vogue there is actually in force.

423. There is another interesting point arising out of the command of Moses to his people. Do the Jews observe the ordinance in the spirit down to these modern times? Have those scattered tribes of the nation who have lived in certain cities of continental Europe for centuries, continued the principle of extruding their excreta from their midst? Have they kept the quarter of the city which has been set apart for them, or in which they have mostly congregated, free from uncleanness? Or have they adopted the customs in this particular of the countries in which they have sojourned? If the former, there should be decided evidence of it in their comparative freedom from the epidemics of dysentery, typhoid and cholera, which have ravaged the cities from time to time in which they have dwelt. If they have faithfully obeyed the law of their great leader, they must have reaped the benefit of it, in immunity from those pestilences from which the people in the other quarters of the cities have suffered. There would and must have been a pronounced difference in the mortality between Jews and Christians in Russia and Poland, for instance, supposing the Hebrew did not allow uncleanness in his own locality. If he has fallen into the ways of the Russian and the Pole and committed the offence of surface defilement, as they have done, he has of course suffered equally with them in all their plagues and he has paid the penalty for his disobedience. Yet I should expect otherwise. The matter is worth investigation; for it might bring out some very curious results, as regards the fœcal origin of disease and the question as to malaria being native or foreign to a soil. The bills of mortality of Hamburg during an epidemical year or two might prove extremely interesting and instructive, or the statistics of any large city in which Jews dwell: always supposing they have adhered to the Mosaic law, if not in its integrity, at least in its principle. Is it not a fact, by the way, that the Jews in the Ghetto at Rome have escaped the ravages of cholera when it has come upon that city? I have heard the thing spoken of and wondered at, seeing the apparently

deplorable hygienic condition of that densely crowded and closely built quarter. If this was the case, I draw two inferences. The first is that the Jews obey the Mosaic law: The second that to their obedience, they owed their preservation. [Corollary. Fœcal matter is the sole means by which cholera spreads.]

424. But now to come back to modern camps—to tent-life, like that of the gold digger, or diamond seeker. The law of Moses would apply admirably to a "rush." Nothing could be better fitted for a temporary occupancy of ground for mining purposes. It would fulfil every condition required to ward off dysentery and other camp diseases for a time;—until the diggings were proved in fact. But though the Mosaic law would, in reality, be the most sensible and salutary hygienic provision diggers could adopt for an emergency such as a rush, it is about the very last thing they would be likely to listen, or conform, to. It must take many years to educate people into the belief that their excreta are rank poison when left on the ground. The common mind cannot of course take in such a proposition all at once. Therefore anything like an efficient excrement-disposal system at new diggings is out of the question for this generation. I do not see any practical and practicable plan by which sickness and death are to be prevented at a rush. By and bye, perhaps, when the men who are recognised as leaders of their fellows become thoroughly alive to the fact that a rush means certain death to a proportion of those present—and they themselves possibly may be among the number of the poisoned victims—it is not unlikely that rude but effective expedients may be evolved from some fertile brain, by which the present deadly habit may be shorn of most of its danger. Some rough laws will then probably be enacted among the miners themselves for their own protection. The main difficulty will be the ridiculous aspect of the question. This of course will be an obstacle to the prevention of wholesale poisoning for some time to come. However, grim death stalking in amongst a family, and a startling incident or two among their immediate friends or relatives, will gradually change this feeling. Men will not long be disposed to make light of the subject, when it is brought home to them tragically in this way. So that it is not improbable that ten years hence, even, there may be a disposition to treat the matter more seriously. The coarse, the brutal, the turbulent, and the criminal, who are always part and parcel of these new diggings, will not be restrained by any considerations as to poisoning others; and I cannot suggest any means by which those who may unhappily be associated with these classes can control them. There must always be a certain amount of danger attending these rapid collections of men in the bush. Conventional rules and sanitary laws will not reach them. They are beyond the pale.

425. But there is a vast amount of local dysentery and typhoid, or "colonial" fever in the more settled districts of this colony of Victoria, which is clearly preventible, and which a very little trouble on the part of those chiefly interested would prevent. At least I speak confidently as regards dysentery, and with scarcely less confidence as regards typhoid. A want of knowledge of a definite kind is

the only thing which has hitherto led to the occurrence of these separate, independent, and circumscribed attacks of zymotic disease. They have not been wide-spread, as they have been dependent upon purely local causes; but in the aggregate they have mounted to a large mortality in the last few years. On the old established diggings, the miners, having no fear of the consequences and, in fact, taking no thought in a matter which they have always believed to be perfectly harmless, have been accustomed to leave their excreta in deserted shafts and claims, or on the surface in secluded places. These depositions of fœcal matter have no doubt caused the death of many of those who have been working or living in the vicinity of the old claims. As the men who have contributed in this way to their own and neighbours' dysentery or typhoid fever, or possibly diphtheria, have not done so wantonly, but unconsciously, it only requires that they should be enlightened as to the danger of the practice and the evil will very shortly be suppressed. When they understand that what they have hitherto looked on as an innocuous habit, may carry death with it all round, they will soon discontinue it. Some of the thick-headed will continue to disbelieve in the danger, long after everybody else is convinced, but public opinion will be too strong for them to defy it to any serious extent.

426. There is a double danger to the miner in surface-pollution, and in the deposition of excreta in old workings. Not only is the air poisoned, but the drinking water may be tainted with the germs of the flux, or the fever, or both. Where the digger procures his potable water from the low ground at the foot of gullies that have been worked, and in which excreta have been left, there is always a possibility of fœcal matter finding its way down to the water-holes. But storm-waters are more likely still to convey fluid excrement, and any fungi that may be connected with it, into his water-supply. The remedy for all this is now, or soon will be, obvious and simple. The managers of all large companies, whether of quartz-mining or deep alluvial, will find means to keep their claims sweet and free from deadly taint; while the individual miners will learn to protect themselves and their families in their own huts or cottages. As the only thing required is to cover the excreta with loose earth, so that none of it shall be exposed, or drain into drinking water, or be washed into it by showers, there is no great mystery or difficulty in the matter.

427. The miners are not the only people who suffer from their excreta. The farmer, the small settler, the shepherd, the hut-keeper, and, in short, all the denizens of the interior of the colony, occasionally fall victims to ignorance of the value and importance of the Mosaic law. Small endemic outbreaks of dysentery and typhoid are heard of in every direction at farms, market-gardens, and out-station huts, and on the townships. This comes of importing the bush rule as to excrement-disposal among dwellings. Dysentery and typhoid, together, tell of surface-pollution. Typhoid, or colonial, fever, by itself, is generally the result of a sodden condition of the soil, for which the privy system is mostly responsible. The ordinary privy of the country is a hole dug in the ground, over which a seat is placed. Nothing is done, or could well be done, to

restrain the semi-fluid material from oozing out into the surrounding soil. So long as this is kept from the atmospheric air it does no harm; but when the fœcal matter finds its way to an exposed surface, the typhoid germ is extremely likely to be developed. But besides this source of typhoid poison, there are many others, as has been shown; especially manuring with nightsoil and leaving it on the surface until it becomes mouldy. The common privy, however, does the most mischief—all of which might be averted by placing any kind of water-tight receptacle under the seat, removing it before it overflows, and digging the fœcal matters into the soil—in the way excrement is now disposed of in the neighbourhood of Melbourne. The only care required is to see that the material is buried sufficiently deep to prevent the access of air, and to preclude its being washed out by heavy rain. By this simple, but efficacious, means, I believe that the bulk of the sporadic cases of typhoid that occur, not only in this colony, but all the adjoining colonies, would be prevented. And, moreover, I have a very strong suspicion, amounting almost to a conviction, that diphtheria would be rooted out by the self-same means. I have said that I am unable to conjecture the conditions under which the diphtheria germ is formed. Yet after looking into the history of the disease, and after attentively considering its phenomena in those parts of the world into which it has followed Europeans, I can come to no other conclusion than that it is in some way connected with excrement disposal. Surface-pollution does not appear to be concerned. It strikes me that the privy system has more to do with it; and I suspect the germ to be a fungus developed on the excrement of privies, or on the fœcal overflow of, or drainage from, privies, under special conditions. Certain it is the city of Melbourne has been much freer from this malady the last three or four years, since the night-soil removal plan has been adopted. A diphtheria mildew on excrement is my view of the causation of the disease; but it is a view without the solid foundation there is for the other hypotheses. If they be demonstrated to be right, the chances as to the diphtheria mildew are greatly increased. In any case, whether the reform in privy arrangement roots out diphtheria or not, it is no trifling matter to get rid of typhoid fever. And that every colonist whose residence stands well apart from the residences of others, may protect himself and his family, to a very great extent from dysentery and typhoid by the means suggested, will, I believe, before many years, be proved to demonstration.

428. The deinfection of cities and towns as regards dysentery has been sufficiently indicated throughout these pages. Simple decency and surface cleanliness, such as obtain in all Anglo-Saxon countries, are sufficient for this one object. Yet, having a regard to other zymotics, this must be combined with a more complete system of some kind. All the large towns of England are just now on the point of altering the old privy and midden systems, which have been sources of typhoid and fields for cholera. Whether the costly experiments some of them are about to enter upon will, or will not, lead to unseen and undreamt of dangers, seems to me to be very uncertain. And until the germs of zymotic diseases are discovered and their

substrata made manifest, some of the proposed plans of "defecation" [or deinfection of fœcal matter] strike me as being extremely hazardous. That they are all empirical is perfectly evident. It is not so evident that they are all safe. It is problematical, I think, whether the average British farmer can be safely entrusted with the disposal, on a large scale, of a material capable of causing the death of scores, or hundreds, of people, if any mistake should occur in the mode in which it is dealt with, or should any unexpected change take place in the weather. Even if the precise dangers to be avoided were all known—if the English understood the practical way of utilising excreta without incurring risk, which it would seem the Japanese have somehow come to understand—some supervision would still be called for to see that those who may be supplied with liquid fœcal manure in the future, do not run into those dangers. A very slight difference in the system of applying excrement to the soil, makes an enormous difference in the result, as may be seen by contrasting the Japanese with the Chinese systems. It may yet be discovered that the climate of England is far more malarious in particular spots than had theretofore been suspected.

429. The allusion to the Chinese reminds me that it has lately been mooted to introduce coolie labour into England from China. If anything comes of this, and large numbers of these people should be collected at any one spot, I predict that epidemic dysentery will reappear in England in spite of its hygienic condition, unless some watchfulness be shown by the Health Officer of the district.

430. There is one matter in connection with deinfection to which I have not yet alluded, but which demands a few words. I refer to a modern plan of excrement-disposal—the earth-closet system. I have no large amount of knowledge upon the subject. Theoretically the earth-closet is perfect. It is the Mosaic law made easy. How far it is applicable for general use is another matter. I observe that great discussion has taken place as to the merits and demerits of the invention. It is not my intention to enter into it. I can quite conceive that the earth-closet may be a great convenience or a decided nuisance in a building, according to the way in which, and to the numbers by whom, it is used. It may be kept sweet and wholesome, or it may be made a dangerous means of inducing disease and death. The ultimate disposal of the excreta from earth-closets is a point worth consideration. If these are thrown in a heap without regard being had to exposure of the fœcal matter, or without care being taken to prevent its subsequent exposure from rain, or to obviate the drainage of fœcalised fluid from the mass, danger may come of it. But if the excreta from earth-closets are dug into soil, with the simple precautions before alluded to, they may be considered safely deinfected. The main principle to be borne in mind—the great object of all deinfective measures—the end and aim throughout—is, according to my view, to prevent the formation of moulds and mildews on the excrement. Anything, therefore, which shall effectually frustrate the production of fungi, I consider efficient deinfection. Nothing that does not answer this purpose, appears to me to be sound, or effective, hygiene. But the hypothesis of dein-

fection hangs upon the fate of the other hypothesis of causation. I may be all wrong; in which case a fresh departure must be taken. For whatever the causation may depend upon, scientific hygiene demands a full knowledge on the subject.

431. It occurs to me here that I should disclaim the use, in the ordinary sense, of the terms "disinfectant" and "deodoriser" and "antiseptic," in speaking of the properties of earth. I have employed these words here and there in their conventional meaning; but it will probably have been understood from the nature of my views that I do not consider the earth antiseptic, deodorising, or disinfecting, in an absolute, or chemical, sense. To prevent misconception, however, I may as well state plainly that I do not attribute to soil, earth, mould, or any portion of the world's crust, any special characteristics by which it acts as an antiseptic. I look on it as a highly valuable means of deinfecting all organic matter, because of its universality and the readiness with which it can be turned to account. But it appears to me that the principal, if not the only, title the earth has to be called an antiseptic, is that it excludes the external air from organic matter. Its chief virtue, I suspect, is a purely mechanical one. I cannot learn that it contains anything by which it neutralises a septic poison in organic matter, when a septic poison is present in it. Chemists have not demonstrated, to my knowledge, that any combinations, or changes, take place in organic matter under earth, that would not equally take place under pure sand, or diamond dust, or even sawdust. The principle of excluding atmospheric air being observed, I opine that any finely divided material would be almost, if not quite, as effective an antiseptic as earth. Certainly it would be as effective a deodoriser. These hygienic terms indeed are somewhat loose and inexact, and do not convey any definite meaning; or, if they do, the meaning does not convey the fact. When I have used them, it will be understood that I have used them provisionally and under protest.

432. Out of this question of the antiseptic properties of earth, crops a very large and important one in hygiene: viz.—What is the law which governs the destruction, or the reduction to inertness, or the defœcation, or the deinfection, of fœcal matter in soil? How long, for instance, will it take for a single evacuation, under given conditions, to become innocuous? And how long would it take an acre of the excrement-sodden soil of Liverpool to get clear of its fœcal matter, supposing no further additions to be made? Or how long would it be before India would be perfectly free from all danger connected with fœcal matter, supposing the present races were turned out of the country to-morrow? Will soil right itself—or rather will fœcal matter in soil, if undisturbed, undergo changes by which it ceases to be fœcal matter? If so, what is the law, or what are the laws, which regulate the processes of disintegration of fœcal matter?

The subject is a complex one and there are few data. As I hold, however, that fœcal matter is effective in the production of malaria only when it has a free surface exposed to the air; that the pollution of water is caused by germs from free surfaces, and is depen-

Dr. Lichtenstein does not refer to sickness or mortality among the officers, as he probably would have done if it had been marked. The presumption is that their tents were to windward and that the men were restricted to one quarter of the sandy plain. The cavalry and volunteers, who suffered less than the other regiments, were no doubt also posted more advantageously, otherwise they would have been affected proportionally with the Germans and the Hottentots, their advantage in physical condition, and superiority in other things, notwithstanding.

All things considered, I take this Dutch case to be the most complete inferential proof of the causation of dysentery by fœcal matter that can well be imagined. Reading it by the reflected light of all other instances of the occurrence of the disease in other parts of the world, after exhausting carefully every other alleged element of causation, I do not see how any other mode of infection could have been the efficient one in this instance. The proposition that fœcal matter was the cause of this endemic outbreak at the Cape, appears to me to be unassailable. I contend that no factor, or factors, that have ever been suggested as the cause of dysentery, will bear investigation here; and that fœcal matter is the only agent which will be found to supply every desideratum demanded by close reasoning. There may be some flaw, somewhere, in the induction, but as I cannot detect it, I must leave the proposition to undergo the crucial test of the future. One proposition I feel quite assured the future cannot disturb; namely, that whether it be finally conceded that surface fœcal matter was the efficient cause of this outbreak of dysentery, or not, no other cause heretofore assigned can possibly be accepted as the efficient cause. Nothing can shake this proposition. I hold it to be absolutely proven. So that if excrement was not the efficient cause, the efficient cause has never yet been thought of; and—*tabulâ rasâ*—the etiologist, or epidemiologist, must begin afresh.

434. Before quitting this most interesting record of camp diseases, I must invite attention to some further highly important conclusions respecting the affection affiliated with the dysentery. What was it? What could it have been but typhoid—typhoid imperfectly described, perhaps, but still clearly pourtrayed as regards leading characteristics? Typhus is out of the question. The first point to be singled out is the causation of this typhoid fever. And where can the enquirer look but to excrement? Water is cut off. The water-supply from the mountain cannot be impugned. The medium of infection, therefore, was air, and air only. And what but excrement can have infected the air efficiently in this instance? There was clearly no indigenous malaria—that extremely problematical, though convenient, property in alluvial soil. There was not even a taint, or a suspicion, of miasm. All the stock reasons for typhoid, in fact, are absent here but the one. And to that one it is driven home with a degree of probability amounting very nearly to certainty. The germ only is wanted to complete the inductive proof. And a typhoid mildew on excrement seems to be inevitable.

435. Assuming the germ from excrement, the conditions as to the development of these germs of the fever and the flux receive a little light from the Dutch camp. Taking into consideration the periods of incubation in the two diseases, it will be patent that the formation of the typhoid germs took precedence of the evolution of the dysentery germs. The daily evacuations of the force by the end of the month would, of course, have exhibited fœcal matter in different stages of change, corresponding to the time of deposition. It seems to be a sound conclusion, therefore, that the typhoid germs come first in order before the flux germs. And this earlier growth, or greater facility in germinating under similar conditions, may serve to explain many of the phenomena in connection with the appearance of the two diseases. Thus it may be conceived quite possible that, after the typhoid germs have been generated and their sporules and other parts sent into the air, some local or seasonal conditions may intervene to arrest the formation of the dysentery germs. Thus the fever is not accompanied by the flux. Again the conditions which may admit of typhoid vegetation, may preclude dysentery vegetation from the first. The typhoid mildew certainly seems to be the more readily engendered and the hardier and more robust and more tenacious plant—or one capable of living under conditions where the other would perish, or of starting into life where the other could not be created. This may be concerned in the phenomena observed in connection with altitude. But there are too many points of interest in this direction, and I must press straight on; merely pausing to observe that the paucity of dysentery germs, as compared with fever germs, at Dutoitspan and Bultfontein on the Diamond Fields, may have been dependent on this unknown law of evolution.

436. The horrible tale of our disasters before Sebastopol has been told so often, and in such graphic language, that I need not linger long over this painful illustration of the effects of a want of knowledge of the causation of camp diseases. It will be sufficient for me here to bring out such facts as point to the chief agent of infection. The first question that arises is—was there a sufficiency of fœcal pollution to account for the amazing loss of life by zymotic disease in the besieging forces? Upon this point I think there should be no great difficulty in forming a conclusion. Narratives for the general reader refer to the feculence and the filth of the camp, and medical authorities speak of the organic matters in a state of decomposition in all directions. In an account of the "Diarrhœa in the Crimea," by Dr. Muir, mention is made of a peculiar affection of the bowels in which the evacuations were passed suddenly;—so that "The "encampment of each regiment was each morning thickly dotted "with evidences of these unseasonable calls of nature."

But independently of all written accounts of the state of the camp, I have made enquiries of some of those who were present as to the degree of fœcal pollution, and I learn that the camp was abominably filthy. In the saps and parallels, especially, it appears that when the troops required to defecate, [and, as most of those living in such an atmosphere were certain to have more or less diarrhœa, this want was frequent] they retired to an angle, or corner, of the works, and passed their evacuations on the surface. The result was that the accumulations soon became most overpoweringly offensive. The stench all through the saps and parallels was nauseating and well nigh unbearable. In the warm moist weather, particularly, the sickening effluvia that rose from these open sewers were perceptible everywhere. And not only were the armies exposed to the malaria from these sources, but when the rains came, the excrement-disposal system in vogue throughout the encampments, proved quite inefficient to prevent liquid fœcal matter from permeating the soil in all directions. In fact the ground was sodden with excrement round about the tents from one end of the camp to the other. This is the picture as it has been presented to me. It may be overdrawn, but judging from results I can quite credit it.

There are no salient points in connection with the causation of dysentery at Sebastopol—nothing that specially illustrates the origin of the disease. There was just the common and inevitable result of a siege, without anything to distinguish it from a siege in Marlborough's time as regards the outcome in disease—except the accidental advent of cholera—and perhaps the larger mortality. I have referred to this instance of camp pollution, in order to point the remarks I shall have to offer on the deinfection requisite to prevent similar disasters to those at Sebastopol. For it will be evident to those who have followed me so far, that I am clearly of opinion that every life lost in the Crimea by zymotic disease, might have been saved by a timely knowledge of the true principles of disinfection.

437. When Omer Pasha marched from Soukhum Kaleh with the intention of relieving Kars, it will be remembered that he advanced to a certain point, where his further advance was stayed by a swollen river. The rains had commenced, the floods had come down, and the torrent was impassable. A retreat was ordered, and the army fell back on Redout Kaleh, the nearest point on the Black Sea. The route taken by the retreating force was, for the first five or six days, the same by which it had advanced. It returned over the same ground. Dr. Dougan Bird, of Melbourne, who was on the British Medical Staff, and attached to the Ottoman Army as Surgeon in Charge during the Trans-Caucasian Campaign, has given me the following interesting particulars of the camp diseases during the expedition :—

Five weeks were occupied on the advance route ; and during the whole of that time the weather was perfect. No rain fell until the last three days of the advance. The sanitary condition of the force was excellent—not one case of illness having occurred. When the Ottoman Army was stopped by the flooded river and Omer Pasha resolved on falling back, the rainy season had set in and the country became difficult to travel through. No sooner did the force set out on the return route, than dysentery and low fevers broke out with singular virulence. The army, which had been hitherto perfectly free from all zymotic disease, was suddenly attacked with the flux and the usual camp affections in all their forms. It is unnecessary for me to refer to the great mortality that occurred in this ill-starred expedition, or to allude to the starvation and scurvy that assisted in the work of death. The interest now all concentres in the one point of causation. How was it that this army, which had escaped dysentery so long and so far, should have been seized upon by the flux immediately on turning back? There should be no great difficulty in apprehending the cause. Clearly the depositions of fœcal matter which had been left on the advance route, had arrived at the state to infect the air with abundance of dysentery and typhoid germs; and when the troops marched back by their former road, they entered into a region made malarious by themselves. The rains of course had favoured the development of the germs; and there were all the conditions for an epidemic. The water-shed too had probably polluted the water supply, and the army no doubt was largely infected in that way. So that slowly toiling back at the rate of about eight miles a day, exposed day and night to an atmosphere laden with emanations from fœcal matter, and condemned to drink water poisoned with excrement mildews, (converted into aquatic vegetation) it is no great wonder that the force was more or less infected throughout. Can any other efficient cause of dysentery and typhoid be substituted for fœcal matter in this case?

438. In one sense the most remarkable exploit, without exception, upon record, was the incursion into Africa by Sir Robert Napier. There is no instance of a force advancing and returning by the same route without camp fever and dysentery to a large extent: that is of course when the operations have extended over a few weeks. From a military point of view, there is perhaps no parallel case; but there are numerous historical accounts of marches and counter-marches of an army over the same ground; and, which amounts in effect to the same thing, of the retreat of one army and the advance of the hostile army in its footsteps; most of which cases may be considered as parallel cases to the African Expedition, from the disease-causation point of view. The return of Omer Pasha's army is a case in point. In none of these instances will it be found that bodies of men passing over ground which had been occupied shortly before by other bodies of men, or by themselves, have escaped the ravages of the flux, or the fever, or both. Until the African case this had been a constant. The degree of malignancy and the extent or spread of the infection, had been a variable result, governed by the local and seasonal and other conditions. But more or less zymotic disease had been a constant. The disembarkation of the troops, therefore, at Annesley Bay, without loss from diseases contracted in the country through which they had passed, demonstrates the fact that dysentery and fever are not a necessary consequence of an army retracing its routes. As I have already had some observations on this unique instance of success, I will only remark here that the sanitary measures adopted in this expedition reflect the very highest credit upon whomsoever were concerned, either in their conception, or in their practical working. They show to demonstration what may be effected in the way of the prevention of camp diseases; while they point most significantly to the actual causation of camp diseases. Had the same knowledge, or the same hygienic principles, been brought

to bear upon the camp before Sebastopol, such a loss of life as occurred would have been simply an impossibility.

439. Marshal Saxe has a very pregnant observation touching camp diseases—so curiously valuable, indeed, that I must quote it. He says :—

"I cannot omit taking notice here of a custom established "amongst the Romans, by means of which they prevented the "diseases which armies are subject to from change of climates, and "to which also a part of that amazing success which attended them "ought to be attributed. The German armies lost above a third "upon their arrival in Italy and Hungary.—In the year A.D. 1718 we "entered the Camp at Belgrade with 55,000 men. It stands upon "an eminence; the air is wholesome; the water good, and we had "plenty of all necessaries; nevertheless, on the day of battle, "which was the 18th of August, we could muster only 22,000 under "arms; the rest being dead or incapable of acting.—I could produce "many instances of this kind which have happened amongst other "nations, and can only be attributed to the change of climates; but "the use of vinegar was the grand secret by which the Romans "preserved their armies; for as soon as that was wanting amongst "them they became as much subject to diseases as we are at present; "* * * in regard to the manner of using it the Romans distributed "it by order amongst the men, everyone receiving a quantity to "serve him for several days, and pouring a few drops of it into the "water which he drank." * * *

The first point to be considered here is the site of the camp at Belgrade. There does not seem any sufficient reason for assuming that malaria was present in the first instance. Wholesome air upon an eminence, according to the Marshal's description, does not look like a miasmatic or unhealthy site. The water was good too, and the camp was in excellent plight—as regards all necessaries. And yet 33,000 out of 55,000 were dead or sick on the day of battle. Marshal Saxe does not mention the length of time the army was encamped on the eminence, and I have not time to look the matter up; nor does he say anything as to the nature of the maladies which caused such havoc. It may, however, be assumed that they were the ordinary camp diseases—dysentery, typhoid, and typhus, with ague perhaps added. Scurvy also may have been, and most likely was, present to swell the mortality—even although it is stated there was "plenty of all necessaries." Cholera was absent of course. If it had been there, it is a question whether the battle could have been fought at all.

What was the efficient cause of the diseases which more than half destroyed this army? The one assigned by the Marshal, viz, change of climate, may be dismissed. There was no demoralisation of the force from defeat, or from other extrinsic sources. There may have been scurvy; but scurvy does not necessarily engender camp diseases. Men die of scurvy without contracting dysentery, typhoid, or typhus. The water is said to have been good; and as it probably came from the river Save, or perhaps the Danube, it was not likely to have been polluted. The only cause left, likely to have been concerned, is the site. Setting aside Marshal Saxe's opinion, which seems almost sufficient to preclude the view of a malaria indigenous to the soil, the site of the Camp at Belgrade is still there. If the camp diseases, therefore, which occurred upon that eminence, were caused by any natural products arising out of the soil, it is clear the same natural products will be found emanating from the soil to the present time—unless the site has been cultivated, or drained; or unless some convulsion has changed the physical aspect of the country. If neither one thing nor the other has happened, it follows that the site should be as deadly now as it was in 1718. The city itself was, and probably, is as polluted as Servians could make it; but the question is as to the site of the Camp at Belgrade. I have no personal knowledge of the topography, but I gather from various sources that the environs of Belgrade are not remarkable at all events for insalubrity—as they infallibly would be,if they caused disease in a like proportion to that in the Austrian army under Prince Eugene. In fine there can be no reasonable ground for assuming the site of the Camp at Belgrade to have been naturally unhealthy, or that the diseases engendered there were a consequence of the locality. It may safely be concluded, I think, that a similar mortality would have attended the Austrians, had their camp been pitched on any spot in Christendom—or out of it. For I engage to say that, tried by the test of the camp diseases of the period, there is not one wholesome acre of ground in Europe or elsewhere.

I cannot see any other efficient cause for the disease in this instance than the excrement disposal system. The camp was the counterpart of a "rush" on a new gold-field, or the analogue of an Indian village; the only difference probably being, that the pollution was greater over a given area and was nearer to the camp, and therefore the effect was intensified at Belgrade. The use of vinegar as a prophylactic by the Romans will be considered presently.

441. It need not be dilated upon how important it will be for armies to deinfect their excreta daily on the line of march. Military men will see the urgency of the need for this, when once they comprehend the results of inattention to it. For not to mention the possibility that the chances of war may compel them to pass over the same ground, it is contrary to the spirit of European nations to add to the horrors of warfare, by any unnecessary injury to the peaceful, or non-combatant, people of a country. Where circumstances will admit of due hygienic precautions, it will be an indelible disgrace for an army marching through a country to sow the seeds of pestilence broadcast. The reward of proper attention to surface cleanliness was reaped by the army which Sir Robert Napier led into the interior of Africa. For can anybody suppose that if the force had returned by a route which had been left in a polluted state, the expedition would have been so eminently successful? If Sir Robert Napier's army had doubled back, as Omer Pasha's did, upon a line largely contaminated by its own excreta, there must have been a very different result. This expedition into Africa and back marks an era in the hygiene of war. For though Sir Robert Napier mentions in one of his despatches, that illness was rife, and that between 200 and 300 men were down on the sick list, yet the well-known trifling mortality of the force throughout the expedition, shows that the maladies of the troops must have been trivial; probably chest affections from the exposure, or diarrhœa, the antecedent to infectious disorders.

442. With regard to siege operations, I will take that of Sebastopol to convey my views of deinfection. The picture of our losses there, from a nescience of disease causation, has been so indelibly fixed by the photographic light thrown upon it by "The Times' Correspondent," that no middle-aged Englishman requires to be reminded of details. I see now that the real secret of our disasters has never been hit. It was not suspected at the time, and if a gleam of suspicion has dawned upon the world since, it has not been followed up and laid bare. I have no hesitation in expressing my conviction that the sacrifice of life in the Crimea by disease, depended solely upon a faulty excrement-disposal system; and that it might have been entirely prevented by hygienic contrivances and a properly directed plan of removal of fœcal matter. I recollect how we were horrified by the accounts, brought out to this country by each succeeding mail during the siege, of the vile commissariat arrangements which debarred our force from procuring proper or sufficient nourishment, and decently warm clothing in the winter. I recollect how our younger blood used to boil at the mess-tables of the Government Camps on the Gold Fields, when the consequences of the brutal neglect, or blundering stupidity, of these army vampires in the Commissariat, were pointed out in the press. The thing was too plain to be doubted for a moment. Bad food, and not enough of it, and scanty clothing, led to innutrition, scurvy, debility, exhaustion, diarrhœa, dysentery, low fever, It was too evident that there was an unbroken chain from raw coffee and salt meat to the flux and typhoid. For the thousands that fell without a wound, the commissariat was

held responsible; and men ground their teeth and wished Lord Raglan were Picton.

The medical world inveighed loudly against the consignment of men to the tomb, made for them by the shameful omissions of the purveyors of the necessaries of life. Yet when the blame comes to be readjusted historically, I suspect there will be some shifting from one door to another. For granting that scorbutic tendencies were shown in the British force, owing to imperfect arrangements, it is not clear that one English soldier in the whole army died from scurvy, or could have died from scurvy, as a necessary consequence of the commissariat arrangements. It is no doubt true that scurvied men are powerless to resist the attacks of specific zymotic disease, and succumb rapidly. But that is another matter. It does not follow that they would have died in the Crimea from scurvy—in fact, I believe death would have been rare from that cause, even if it had happened in one instance. Scurvy is certain death, of course, when the total deprivation of vegetable matter is prolonged beyond a certain point. But it takes some time to reach that point; and I doubt extremely whether it would have been reached in the Crimea. But for the intervention of the zymotic diseases, then, the troops would not have died. And who is responsible for the development of these diseases within the camp? Who may be legitimately supposed to have had the sanitary measures under control? Not the Commissariat assuredly. That well-abused—and perhaps deservedly well-abused—department must clearly be relieved from the stigma of the disease carnage in the Crimea. It is idle to say that men died more certainly, being scorbutic, or were attacked in larger proportions by dysentery, fever, and cholera. This may be true, but it does not affect the proposition as to the production of the zymotic diseases. Except for them, the men would not have died at all; and from them thousands of men would have died, although they had not suffered from the least taint of scurvy. It does not require the presence of scurvy to make camp epidemics largely fatal. If this be so, let not the evident palpable sins of those whose omissions led to scurvy in the Crimea, be confounded and burdened with the unseen and unsuspected sins of omission of those, whose want of exact knowledge led to the causation and propagation of diarrhœa, dysentery, typhoid, typhus, and cholera.

443. If every particle of fœcal matter voided in the Crimea had been carefully collected, sealed hermetically, and shipped to England, the country would have been the gainer by some thousands of lives and millions of money. What, however, was essential, was the complete deinfection of excreta on the spot. This might have been accomplished, either by complete periodical removal to a distance by a system of receptacles admitting of being emptied into carts specially provided for the purpose, or by the trench plan of disposal. The great difficulty connected with the latter, no doubt, would have been the rains. Practically it would have been almost impossible to have prevented fluid, holding fœcal matter in suspension, from escaping from the broken ground. And even solid fœcal matter itself might have been washed out, or exposed, by storm waters. One of the

448. If naval architecture has reached its highest point in the construction of these cloacal parts of a ship, the evil must be borne; but I apprehend that ingenuity is equal to fashioning less deadly contrivances. And when the causation of zymotic diseases shall be more clearly understood, the imperative necessity of inventing some other mode of excrement-disposal for ships of war will be admitted and acted on. The question is a much larger one than may seem on a superficial glance. A collection of fœcal matter, though a slight one, might lead to the most unfortunate results on board a ship. An endemic attack of dysentery, or diarrhœa even, supposing it to be limited to two or three men, may be the means of causing fearful mortality, as I have no doubt it has, frequently, among a ship's crew. A ship having her full complement of men, with 500 or 1000 troops on board in addition, is in a perilous position in any weather. The ammoniacal stench of the highly nitrogenised air below at night, is described as being something horrible, in the roomiest and cleanliest of ships. All that is required—as I conceive—to breed a pestilence of typhus, or an epidemic of dysentery, among men so placed, is the introduction of dry fœcal matter. And what so likely to ensure such a result as dysentery? Setting aside scurvy, which is hardly to be thought of in these days in a British ship of war, dysentery, or diarrhæa, is the most probable means by which the one thing only wanting to induce typhus, is left between decks. Cholera also is another outcome of the faulty system of the "head" in ships. It is notorious that the officers of men-of-war have always escaped the attacks of cholera which have ravaged the crews. And what does this exemption point to? It cannot be attributed to physical condition, or difference in food. For, not to take the point that the healthiest of men are liable to contract cholera, the food of the common sailor in the Navy is quite as fitted for the wants of the system as the food of the officer; and the physical condition of the latter is in no way superior to that of the former. So that, as the two classes stand on an equal footing as regards receptivity of the poison, the only assumption is that the one class is not exposed to infection, by having to inhale cholera germs. There can be no difference in the water-supply. Where then can the explanation of the phenomenon be looked for, but in the excrement-disposal system? And how about malaria from decomposition of organic matter in alluvial soil in connection with the development of zymotic disease on board ships?

It appears to me that dysentery, typhoid, typhus, cholera, remittent, yellow fever, and plague, may all be engendered, or find a substratum for their germs, in consequence of the "head" system. There may possibly be depositions of fœcal matter in the chains, or boats, at times, during rough weather, or adhesions of the material to the outside of the ship occasionally: yet these accidents must, or should, be of rare occurrence. When they occur in warm latitudes, they must be almost certain to end in diarrhœa, or dysentery. The principle of the deinfection of all ships is so self-evident that it requires no further remark.

DISINFECTION IN RELATION TO DYSENTERY.

449. As regards dysentery, disinfection is a very simple matter, both in principle and practice. It is by far the easiest of all the zymotic diseases to prevent from spreading: and I venture to say this, although the authorities are ranged against me. In civil life in an epidemic of dysentery in any country, the first thing a wise man will do is to get away if he can. Those who remain have one grand resource to stop the plague—and only one. They must sweep every particle of excrement out of the locality, and the disease will disappear. If they do not choose to do this, and to keep their town or city free from pollution ever after, there is no more to be said.

450. Disinfection of the sick chamber in private houses, is unimportant where due cleanliness is observed. The only material point is to prevent any of the discharges from drying on the bedding, or elsewhere. The disposal of the excreta is of more importance—although they are not nearly of so dangerous a nature as is supposed. If thrown into a water-tight privy, for instance, I believe they would thus be safely disposed of. If, however, they come into contact with exposed fœcal matter afterwards, the germs may be propagated; and should they find their way into drinking water, the pollution of the water will continue so long as there is a supply of organic matter for the germs. All things considered, it is safer, therefore, to dig the excreta of dysentery patients into soil with more than ordinary precaution. Among the poor and crowded quarters of infected towns, of course the same principles apply, but the nations and people who generate epidemic dysentery, are not very likely to use the necessary means to prevent its propagation. If they will not deinfect, they will not disinfect. As I have already referred to hospitals, I will merely offer one or two more remarks on military disinfection.

451. Dysentery in camps requires to be most carefully disinfected, both within and without the hospital; and this not only to prevent the flux from spreading, but also to avoid the chance of starting typhus. In a letter to the Lancet, I observe Professor Maclean quotes Sir John Pringle on this point. He, it appears, "insists strongly on the destruction by fire of straw that has been "used in camps where this disease (dysentery) prevails." And a most necessary precaution too. Nothing is more likely to produce dysentery dust than old straw; or to conceal particles of dry excrement for the mould of typhus to form upon. With every care, however, dysentery and the camp fevers must always be reckoned upon as among the chances of war. There are certain phases that will present themselves in every campaign, in which these diseases must inevitably be engendered. The victors and the vanquished will each be exposed to infection. The only thing to be done therefore is to lessen the evil as much as possible, by combining deinfecting measures with disinfection.

245. I will now refer to the subject of the use of vinegar by the Romans, spoken of by Marshal Saxe [439]. Gibbon alludes to the

power the Romans had of acclimatising themselves in any country, but he does not give any explanation of their mode of effecting it, except incidentally by discipline. The first question that arises is—were the Romans able to maintain themselves in bodily health in their camps? And secondly —if so, was it owing to the use of vinegar? I must leave the question as to the fact of the Romans having acquired the art of living healthily in camps, to those having learned leisure. If it be shown they had the art claimed for them by Marshal Saxe, it is quite clear the use of vinegar was not the whole secret, though I am not prepared to say that vinegar is not a valuable remedy in some camp diseases. In the first place the Roman soldiery did not get vinegar, as such, served out to them, as it would seem. They got an inferior wine called *Posca*, made from lees after the first must had been poured off. This *Posca* was a thin, sour, wine; but it was not vinegar, necessarily, when served out to the troops, though it probably changed into it before very long. If it be true that the Romans suffered as other nations when their supply of this liquid ran short, it is a very extraordinary fact: but I think it requires searching investigation before it can be accepted. On the whole I am more disposed to think that it was chiefly valuable as an anti-scorbutic; that the Roman generals insisted on surface-cleanliness in their camps; and that this latter was the main secret of their salubrity.*

453. Yet there would seem to be some peculiar property in vinegar—some prophylactic, or antiseptic, quality, which has not been thoroughly worked out. I am not so sure but this domestic hygiene for the sick chamber has some value. Everybody flies to it instinctively; and, I suspect, with reason. It acts, probably, if it acts at all, as a disinfectant, rather than as a deinfectant; and it may be considered both as an internal, and as an external, agent. Taken internally, vinegar certainly seems to possess the property of neutralising, or rendering innocuous, some vegetable poisons, in a very decided manner. I cannot give a better illustration of this than the fact mentioned by Livingstone in his Missionary Travels in South Africa. In Chap. vi., p. 113, will be found an account of the plant "ngotuané" which contains an active poison. "Vinegar," says Livingstone, "has the peculiar property of rendering this poison perfectly inert, whether in or out of the body. "When mixed with vinegar the poison may be drunk with safety, "while, if only tasted by itself, it causes a burning sensation in "the throat, &c." Given to a French gentleman who had taken an infusion as tea, "the cure was instantaneous and complete." What is the explanation of this phenomenon? Has vinegar the power, when admitted into the blood, of dissolving or breaking up, vegetable cells?—or does it decompose, or form new combinations with, unstable alkaloids? Is it possible that vinegar acts on

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* In note 11, chap. ix., of the Decline and Fall, Gibbon has:—"The Romans made war in all climates, and by their excellent discipline were in a great measure preserved in health and vigour."

the "low vegetable organisms" of zymotic diseases, in either of these ways, or in any other way?" Will vinegar dissolve moulds or mildews?

As a disinfectant externally to the system, vinegar can only be efficient by its vapour—unless when applied to the surface of the body, as is sometimes done by nurses. Has vinegar the property of combining with, and decomposing, disease germs, floating in the air? If so, it might be a valuable adjunct to the disinfection of military hospitals in time of war.

There are many other details in connection with this branch of hygiene to which I intended to refer; but I feel I have already wasted sufficient time in this tentative essay, if the conception of mildew causation should prove abortive.

EPIDEMIC DIARRHŒA.

454. A few words upon this, the well known precursor of all epidemics. Cholera, typhoid fever, and dysentery, are especially ushered in by a prevailing laxity of the bowels of the community; and even those who do not take the specific infection, are perhaps affected more or less before, and during the continuance of, an epidemic, by some irregularity of the intestinal functions. It is commonly said that this indicates a malarious condition of the atmosphere, or an epidemical state of the air, or some other vague and undefined thing. From my point of view the phenomenon is perfectly explicable; and there is no occasion to have recourse to ingenious explanations in which nothing is explained.

455. The local and seasonal conditions being largely favorable to the growth of mildews generally, and mildews on excrement, in particular, the more virulent mildews are preceded by common, rapidly-forming, light, feathery, comparatively innocuous, mildews, which are ever ready to form upon, or to spread over, a fœcal substratum; from which they acquire irritating, or poisonous properties, sufficient to derange the system, when inhaled, so as to induce laxity of the bowels, but not as a rule to set up serious mischief. These light and less important mildews, give place to the sporules of advancing cholera; and the exotic mildew seizes on the substratum previously occupied by the indigenous diarrhœal mildews. This is one hypothesis. Another is that during the first stages of the growth of a large crop of mildews, filmy portions of the vegetation are detached from the partially formed, or unripe, cryptogams, and are borne into the air. These minute particles are given off before the vegetation has acquired its distinctive, or characteristic, properties,—before the plants have developed themselves and produced sporules in fact—and, although endowed with some slight degree of virulence, fall far short of the intensity of the matured virus. They are the *avant-couriers* of the coming infective germs—the aërial messengers of the ripe cholera poison.

456. It seems probable that the two kinds of germs conduce to swell the amount of diarrhœa antecedent to the actual outbreak of cholera. If the common mildews of the country, only, should happen to be concerned in the epidemic looseness, a sudden change in the weather may cut off the disease by dispersing, or nipping, the vegetation. And even after cholera germs have just declared themselves, by the presence of the disease in a community, the affection has been known to disappear rapidly from a high wind, and from other meteorological conditions;—the inference being that the mildews have been disturbed, and their power of reproduction at the spot destroyed. This, of itself, disposes of local alluvial decomposition.

457. It would seem from the comparative mortality of infants, children and adults, that the younger the age, the more deadly the effect of malarias containing portions of cryptogams. Epidemic diarrhœas which do not derange the organism of the adult to any serious extent, prove extremely fatal to infants. Sometimes the diarrhœas of the latter run on to a dysenteric form, even when there is no true dysentery in the locality. That is to say the slimy discharges of the infant, may contain blood and shreds, or sloughs, of mucous membrane, without there being any reason to suppose that the specific dysentery germ has found an entrance into the system. In these cases it looks as though mechanical conditions are involved.

458. As Mr. Radcliffe has pointed out:—"The prevalence of enteric fever is chiefly determined by the same local conditions which "foster diarrhœa, namely, the pollution of the soil in the vicinity "of houses, of the air used within houses, and of the water used "for drinking purposes, with excrementitious matter." There can be no reasonable doubt as to the soundness of this proposition. The mode in which the air is infected by excrement may be resolved into gaseous emanations, particles of excrement substance in some particular state of change, or some form of mildew. There can be no doubt that gases will induce diarrhœa—but not the kind of diarrhœa now treated of [an epidemic purging.] It is quite conceivable that constant exposure to gaseous emanations may end in diarrhœa, without any infective substance being taken in with the gases—especially among infants and children. A malaria of this kind is, indeed, not only conceivable, but has been maintained by many to be the efficient cause, not alone of epidemic diarrhœa, but of other specific disease. For my part I am not disposed to accept it even as an efficient cause of the particular form of diarrhœa now under consideration.

The question as to the infection of air by minute particles of fœcal matter as a cause of epidemic diarrhœa, I think may be disposed of without much difficulty. The only assumable way in which excrement itself could permeate the atmosphere, so as to get into the breathing apparatus, is by subdivision. The dust of fœcal matter must be assumed, and the launching of this dust into the air—perhaps upon the "*rafts*" of Professor Tyndall. But this would involve the supposition of extremely dry weather—a condition antagonistic to epidemics of cholera, typhoid, and dysentery. I therefore suspect that general epidemic diarrhœa, [as well as some local prevalent diarrhœas among children possibly,] depend on mildews, or moulds, upon excrement. At the same time the probability of the extension of these mildews over other organic matter, must not be overlooked.

Although I cannot dwell upon this large and important subject, the mildew hypothesis enables one to apprehend the fearful mortality among children in excrement-sodden localities—more especially in localities where surface-pollution is extensive. For it may readily be understood that low vegetable organisms of the simplest forms, may occur under conditions that exclude a more highly organised vegetation; and may be almost invariably present when such cryptogamic plants as the cholera, typhoid, dysentery, and other, fungi, cannot be created, owing to the absence of some one or more of the required conditions. And these simple vegetable organisms may set up disease in children, though they may pass through the system of the adult, without inducing functional derangement.

457. The diarrhœa of India is a terrible thing for Europeans to encounter. Once set up, there is very little hope of shaking it off, except by leaving the country. Change of air is tried in India itself, in vain, as might be expected; for what is it but a removal from one fœcalised spot to another? Even the "hills" are contaminated, and sufferers from the exhausting diarrhœas of the plains, go up there to change their particular forms of the disease, into what is known as the "Simla trots." And not only is India cursed with malarious diarrhœas, but every tropical and subtropical country, in a direct ratio to the degree of its excrement-pollution. In any one of them you may live healthily enough, if you can only steer clear of the people; but if you have to be among them, you are fortunate if you get off with diarrhœa. The infantile diarrhœa of such places as Liverpool, is merely the European representative of the diarrhœas of hot countries. The excrement-sodden lanes and alleys engender only a mild inert mildew, merely capable of destroying a few thousands of infants yearly, throughout all England; but a larger field and a hotter sun produce more potent germs on an immense scale.

458. While upon this subject, I may allude to another European prototype of an Indian disease—Summer Diarrhœa, or Sporadic, or Bilious, or English, Cholera. What is this but the result of an excrement mildew—a mildew developed under peculiar climatic conditions, analogous to those of the exotic cholera mildew? It is clearly a closely allied species; though, fortunately, less dangerous and more ephemeral. Yet this English mildew is virulent enough to kill on occasion; and the mode of death is, sometimes, so precisely similar to that caused by the Indian fungus, that the highest authorities declare there is no perceptible essential difference in the manifestations. Is it a legitimate supposition that the Asiatic

cholera mildew produces its effects, chiefly by reason of its configuration; and that the English cholera mildew is fashioned after the same plan? If the exotic cryptogam acts mechanically, by causing obstruction to the circulation in the liver and kidneys, by entanglement of some parts of its structure in the small blood-vessels; it may be conceived that the English fungus will produce a like series of results, approaching more or less to similarity, or identity, according as the plant is more or less highly developed, or approximates more or less to the physical peculiarities of the tropical mildew.

459. There is a large variety of specific epidemic diarrhœas in the world, many of which may be traced to errors in the excrement-disposal system, and most of them dependent for their extent, upon the same local and seasonal conditions that precede other epidemics. Heat and moisture are the general preludes and accompaniments, though there is a curious exception to the rule in Africa. Livingstone mentions that, in the Bakwain country, the epidemical seasons usually come before the rains. "Sometimes it is general ophthalmia, resembling closely the Egyptian. In another year it is a kind of diarrhœa, which nothing will cure until there is a fall of rain, and anything acts as a charm after that." If this diarrhœa depends upon a fungus, it should be a dry mould like that of typhus, (?) or one analogous to the *Sclerotium* developed on cow-dung [92].

The deinfection and disinfection for typhoid fever, is manifestly that for the epidemic diarrhœas caused by excrement; that is, supposing it be eventually determined that they both depend on mildews.

CONCLUSION.

The one point I take to have been settled in the foregoing pages is that dysentery is caused by surface excrement. Upon that one point I feel assured. I consider that to have been proven—not in all the logical forms, perhaps, but yet proven. Whatever comes of the more speculative views, I maintain that the causation of dysentery has been shown to be inseparably connected with fœcal matter; and fœcal matter placed under the conditions laid down, or some of them. For I do not presume to think I have worked out all the conditions under which dysentery is engendered by excrement; nor do I venture to affirm that the details of the propositions advanced are all absolutely correct: but I yet submit that the great essential conditions are given; and that the theory of causation is substantially sound as a whole. Having examined the question from every point of view that offered, and having regarded its smallest presenting surface during several months, I can see no other conclusion than that pollution of the exterior of the earth, by excrement, must infallibly be the cause of dysentery. If this be disproved, I shall admit to a defective induction with the greater readiness and satisfaction, if the true explanation of the origin of the disease come with the disproof. At present, since everything I have written hangs upon this one theory, I may be allowed to assert my belief that it is not to be controverted.

The mildew hypotheses that have been built up on the strength of the efficiency of exposed excrement, and of the soundness of the germ theory of disease, I reiterate, I do not consider to be based on so sure a foundation, as the theory of the causation of dysentery. And yet they appear to me to have such an amount of probability, that I regard them almost as a certainty. All that is required to convert them into inferential proofs, is the discovery of one vegetable parasite on excrement, capable of causing any one of the specific diseases named. If dysentery, for instance, were shown to be caused by a mildew on superficial aërated fœcal matter, I do not see any possible escape from the deduction, that all the other diseases in the class come from vegetable germs, developed on the same substratum under other, dissimilar, or varying, conditions. The analogies are so close, as scarcely to admit of the intrusion of a doubt. The one hypothetical mildew demonstrated, abstract reasoning leads directly to the other hypothetical mildews; and, indeed, their actual demonstration, in every case, need not be waited for, in order to convince us of the fact of their agency in the production of their several specific diseases. The only things dependent on the absolute proof of their existence, will be the determination of the periods and the conditions of their evolution, growth, and dissemination; together with the knowledge of their physical characters and toxical qualities, and of the modes by which they cause their effects in the organism. These things will be material and vastly important for practical purposes; but they will not be required to establish and confirm the view of a fœcal cryptogam as the infective agent in some diseases. The philosopher may argue on this basis, as assuredly as though the microscopist had found the fungus, and had formulated all the laws of its production. Science, indeed, may keep ahead of the microscope, and need not be dependent on necessarily slow successive observations and experiments. One thoroughly sustained proof of a mildew growing on the assumed substratum and causing a specific disease, would be a fixed point for safe induction as to allied specific diseases. With this fulcrum and the lever of analogy, the philosopher might move the mildew world.

The reader will, I trust, apprehend the position I have taken. If I have gone into the region of hypothetical fungi and brought out large deductions from assumed data, at all events it has been done openly. I have not hesitated to distinguish between things known and things unknown. If I have made a bold guess, or ventured upon a rash surmise, here and there, it has been declared and may be easily checked. The chances are largely against the final acceptance of all the crude notions here set down. But I have made no pretence to finality. If the premisses turn out to have been incorrectly assumed, the hypotheses depending on them of course are valueless. I am quite prepared, therefore, to witness the destruction of some of the views I have conceived and broached. I cannot expect that a tissue of pure assumption, such as I have been compelled to fabricate, should be free from imperfection throughout the piece. If error be interwoven, I shall not be surprised; but then it will perhaps be remembered in extenuation, that I have been working in the dark, or nearly so. The amount of light diffused by mucologists on the special subject of my enquiry, has been only just sufficient to enable me to see the shadowy outlines of some things; so that I have had to deduce the others. In fact, I have had to construct an imaginary department in botany, and to create an ideal tribe of fungi, where, possibly, none exist. There was nothing else for it—unless I gave up the mildew hypothesis, which was not to be thought of. It will hardly be wondered at, if I shall be shown hereafter to have taken a wrong turn or two, in wandering through the supposititious cryptogamic maze into which I entered without thread.

It will be seen, then, that I anticipate the demolition of some portions of the unsubstantial vegetable system I have framed. I have no doubt that parts of it must go. And the argument depending on those parts must go too. I think, however, some of the structure will survive. If only one or two of the larger deductions as to fœcal mildews stand, I shall be quite satisfied with the result. If, for instance, it be demonstrated that fungi of any kind, occurring on excrement, cause zymotic diseases, it will be something gained. And if, in addition, the problem of the pollution of water with poison germs, shall be shown to have been solved, it will reconcile me to the dismemberment of all the other hypotheses. Not, be it understood, that I think they are all doomed.

The order in which I have taken typhoid, cholera, &c., indicate the degree of probability there is that their causation depends on excrement. Next to dysentery, I conceive the presumptive evidence in favour of these diseases being directly due to fœcal matter, is strongest in the case of typhoid, and weakest in the case of typhus. The coincidence of typhoid with dysentery epidemics; the recognition on all sides that excrement is a principal factor of typhoid; and the invariable detection of this element in the causation whenever it is searched for, all lead to the conclusion that this substance is largely concerned in the origin of the infective principle of typhoid. If to this accumulated testimony, one may add the hypothesis that the infective principle is a vegetable germ, as has been so ably maintained by the Germans and others; and that the vegetable germs come from the source I suggest; and further that the terrestrial mildew is transformed into an aquatic plant, as I have conjectured; it seems to me that not only is fœcal matter connected with typhoid, as the sole factor, but that the mode of its connection is clearly made out. The only thing now wanting to complete the chain is to connect the microscopical fungus with the assumed substratum. If that be not done, and done readily, I am hugely mistaken. When it is done, I think Sir William Gull will have to admit, that there is not only "a good working theory," but a good scientific theory also. If England groans under the malaria which entails typhoid fever, for 250—aye or for 10—years, after the scientific theory is established, I do not think Englishmen deserve the eulogium Sir William Gull passed on them in his Lecture.

The disease I consider the next most certain to be dependent on excrement—and excrement solely—for its origin, is cholera. There is but a shade of difference in the calculation of the probabilities as to the origin of these two diseases; and I take it upon me to affirm that, if typhoid be traced to excrement in the manner described, the causation of cholera must also be traced to excrement—under slightly modified conditions, perhaps, but still under conditions to maintain either the typhoid mildew, or the cholera mildew, as accident may determine.

Yellow fever is next in order as regards probability. If dysentery, typhoid, and cholera, are caused by fœcal parasites—the chances of yellow fever being similarly caused come next in the sequence. They are only less than in the other cases, because the disease has not been much observed in Europe, and the collection of facts is therefore smaller. Two or three of the observations made, however, as to the mode of the origin, or extension rather, of the disease, exclude the possibility of a general malaria, as well as a million of similar observations. It has been demonstrated that a specific infective agent is involved. The only point upon which the evidence has fallen short of that in the foregoing cases, is the one relating to the association of the malady with excrement. On this point less has been said; but there is quite sufficent known for safe deduction. The alternation of cholera with yellow fever epidemics, points conclusively to similarity, if not identity, of causation, with some slight variation in the factors.

The next on the list is remittent fever. If the arguments adduced to establish the compound origin of this disease hold water; and if the febrile part of the malady be the result of typhoid poison; I look upon it that a vast proportion of the remittents of the world will be shown, eventually, to depend on human excrement. The probabilities that excrement and remittent stand in the relation of cause and effect, are only less than those of typhoid being connected with excrement, because that the direct dependence of fever and ague upon human excreta has either not been suspected, or has not been investigated. The varying nature of remittent in different latitudes, also suggests the probability that, in some instances, the non-aguish symptoms are not caused by typhoid germs, but by some other germs, though germs developed upon the same, or a similar description of substratum. Thus the remittents met with in some parts of India, which bear such a close resemblance to yellow fever that many observers have regarded them as identical, are probably not true typhoidal agues, but agues conjoined with fevers affiliated to yellow fever, both in their causation and by their manifestations. Excluding the ague principle, which is not a constant by the way in this form of malady, this East Indian *quasi* yellow fever seems to me to be correlated to the veritable American and West Indian black vomit, as English cholera is to the Asiatic. There is an affinity in the mildew vegetation in both cases, though the fungi producing the spurious fever and the bilious cholera die out rapidly from some cause, whilst those of the true yellow fever are persistent, and those of Indian cholera maintain themselves for a season. I consider the likeness of certain Indian remittents to yellow fever, an additional argument in favour of the former having their origin in fœcal matter.

So many various specific diseases are grouped under the head of remittent fever, when the ague-plant happens to co-exist in the system with them, that it would obviously be useless to attempt to lay down any universal principle that should apply to the causation of all. I consider, however, that the probabilities that, at least, four-fifths of remittent disorders depend on mildewed excrement, are very strong indeed.

Typhus is undoubtedly more obscure, as to its causation, than any of these diseases. I regard the hypothesis I have ventured to submit upon the point, as being immeasurably less likely to prove the correct solution of this intricate problem, than is the least likely of the other hypotheses to transpire to be a sound explanation of the cause of any of the foregoing maladies. I have given a deal of consideration to the subject; but whether I have succeeded in bringing out the real infective agent of typhus, I leave to those in a better position to judge and determine.

Relapsing Fever I have only mentioned incidentally. Its causation is so clearly allied to that of typhoid, that elaboration is unnecessary. It probably depends on a less highly organised mildew than the typhoid mildew. If the one mildew be a fœcal parasite, however, the other is sure to be. The peculiarity of the relapse in this "hungerpest" is obscure. Perhaps the determination of the law of periodicity in ague might serve to elucidate the recurrence of the symptoms. Relapses in typhoid, typhus, cholera, &c., are efficiently accounted for by the resubmission of the patient to the original cause of the disease. He is placed in a position to inhale, or swallow, fresh poison germs; and the whole train of manifestations is brought back again. But this view does not apply to a fever, in which the relapse is a constant, or nearly so. Something else must be assumed. All things considered, I calculate the probabilities that relapsing fever depends on fœcal matter, to be nearly as great as those as to typhoid depending on fœcal matter.

That the present plagues now existing in the East, and occasionally breaking out in Constantinople and in the different parts of the Levant, are purely fœcal plagues, I have no manner of doubt. Nor can I hesitate to believe, that the plagues which formerly ravaged Europe, including our own Great Plague, were dependent solely upon human excrement. The accounts given by De Foe of the Plague of London; by Boccaccio of that of Florence; and by Thucydides of that of Athens; clearly indicate a family resemblance in these pestilences, and that they were of kin to those horrible bubonic plagues still maintaining their hold in some parts of India. The variations that have been observed in all of them, are no greater than those which are found in typhus and typhoid fevers. The plague of Cairo, the "black death" of Italy in Boccaccio's day, and the Maha Murree of Rajpootana, differ no more than epidemics of typhoid differ in England—if so much. In fact the plague would seem to be an intensified Oriental typhus, with, probably, an occasional intermixture of tropical typhoid germs. For Thucydides mentions that those who recovered from the plague, had ulceration of the bowels and diarrhœa. Yet the symptoms, lesions and results, ally the disease more nearly to typhus; and the intrusion of typhoid manifestations is most likely accidental. Typhoid would appear to be merely an accessory after the fact. I cannot now elaborate the subject; but I would observe, that if the history of the former appearance and disappearance of plagues in Europe be rightly studied; and if their dependence on hygienic measures, as has been demonstrated in modern times in Malta, Cairo, and the East, be attentively considered; it will be apparent, that they have owed their causation and propagation exclusively to human excrement. Nor can I see any efficient mode by which the infective agent has been, and is, produced, but by a cryptogamic vegetation on a fœcal substratum. Yet there is a peculiarity in plague epidemics—a marked distinction in connection with their origin and spread; to which I will briefly advert, as it appears to throw some light on the causation of typhus, and supplies an additional argument to the hypothesis advanced as to its causation. From all I can gather, I arrive at the conclusion that the plague, contrary to all the other diseases enumerated, save typhus only, starts as a *dry epidemic.* That is to say it does not commence *de novo*, or spread rapidly, except at times when there is no rain and no moisture in the air. Of course its highly contagious nature suffices to propagate the disease, when once it has got thorough hold on a population, no matter what the hygometric state of the atmosphere may be; but I think it will be found that heat, without moisture, is a constant precedent condition to the commencement, or to the rapid increase, of the bubonic plague. If this observation be borne out by others, it is a reasonable inference, supposing the fungoid hypothesis to be tenable, that the vegetation on the fœcal substratum occurs in the form of a dry mould or mildew—just such a description of vegetation, as I have assumed to be the cause of typhus. By this view the plague would be, in fact, a sort of open air, or out-of-door, created, typhus. Even the Great Plague is no exception to this, as the ordinary climatic conditions of England might lead one to suspect. For the disease was brought to London at the end of November, or beginning of December, 1664; and it maintained a bare existence, on contagion alone, for many months. "But," says De Foe, "those "were trifling things to what followed immediately after; for now "the weather set in hot, and from the first week in June, the "infection spread in a dreadful manner, and the bills rise high." However, I must now leave the plague to others, contenting myself with claiming its germs, by analogy, as fœcal germs.

Of the plagues of boils and the malignant ulcer pestilences of tropical countries, I know but little. But that little induces me to class them in the same category with the other fœcal disorders.

The very singular development of elephantiasis, or leprosy, in New Brunswick, has led me to the conclusion, somewhat unexpectedly to myself, that this ancient disease must be added to the terrible list of penalties man brings on himself. I certainly little thought there was any relation between the two things, when the subject was first brought under my notice: yet, strange and strained as the notion may appear, and as the craze, perhaps, of a man possessed with one idea; I must nevertheless say, I do not see any efficient cause for the disease that shapes so promisingly as the one suggested.

Then there is dengue, which has been experienced largely, of late, in India. Looking at the locality and the nature of the complaint, suspicion points to a fœcal mildew causation.*

But to leave tropical diseases and to come to one which concerns us more nearly. I conceive that the probabilities that diphtheria will be connected with fœcal matter before long, are very great indeed. I am bound to confess, however, that I can form no reasonable conjecture as to the precise conditions under which the poison is produced. The particular mode by which the infective germs are generated eludes me. The phenomena presented to the student of this disease, are so varied, that the problem of its causation is admittedly a very complex one. The history of the affection is replete with curious and perplexing irregularities, connected with infection and contagion. In some instances, infection seems to have been brought about with the most dangerous facility and rapidity. In others, the application of the diphtheritic exudation to mucous surfaces, and even actual inoculation, has not sufficed to infect. At one time prolonged exposure, or seeming exposure, to contagion, has proved innocuous. At another, the malady is said to have been communicated at once, in the purest sense of a *contagium vivum*. Again, the poisonous principle has been so capriciously distributed that, of two families living in the same house, the members of one family have all suffered, and the members of the other have all escaped. Diphtheria has been known to leave some houses suddenly and mysteriously, and to cling to other houses for months, so that those who have left because of the disease have fallen ill on their return. The erratic way, too, in which diphtheria attacks those on the hills above and passes over those in the valleys below, or the converse, occasionally singling out the healthiest looking sites for its most fearful outbreaks, has tended to complicate the question of causation. And the peculiarities of the disease itself, have been another obstacle in the way of arriving at its origin. Altogether the difficulties have been insurmountable.

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* The last advices from the Mauritius bring word that this exquisitely painful, but not dangerous affection, has found its way to Port Louis. This is strong presumptive proof of its fœcal origin.

But now let us look at diphtheria by the light of a fœcal mildew. I submit first that the disease is caused by a vegetable germ derived from a fungus developed on excrement, under conditions which I cannot foreshadow, but which I conceive to be connected, in some manner, with the excrement-disposal system of Europeans. [I will narrow down this proposition presently.] And secondly, I submit that the disease will eventually prove to be not only a preventible disease, but preventible by simple means.

These are bold propositions, and perhaps hazardous. But either I am wrong from first to last, or I am right now. If the inception was bad, each step has been more and more vicious. But a dysentery mildew entails a diphtheria mildew. If the assumption of a dysentery cryptogam was legitimate, the inference as to a fungoid origin for diphtheria, is, as I conceive, a necessity. The one granted, the other must follow.

As regards the former of the two propositions, it may be observed, in the first place, that the lesions in diphtheria are closely allied to those in dysentery. Rokitansky shows that tubular sloughs of mucous membrane are common to both affections; and Niemeyer and others have alluded to this. Professor Hallier has discovered the micrococcus which he claims to be the vegetable germ that causes dysentery. Dr. Letzerich has found a corresponding germ for diphtheria; and although his observations have not been confirmed, or repeated, so far as I know, I accept his statements, for reasons formerly given, and for other additional reasons. I may say, indeed, that even if Dr. Letzerich had not found the germ, I should, nevertheless, have the profoundest conviction that one is to be found. I have assumed that the dysentery germs come from a mildew on excrement under certain specified conditions. I now assume that the diphtheria germs come from a mildew on excrement under certain other conditions, not specified; and I offer the following reasons.

(1) There is no other known efficient cause of diphtheria. (2) General malarias are excluded. (3) The disease is purely local as regards causation, though propagable by infection, and perhaps by contagion. (4) It is essentially an European disease, though met with in tropical and other countries inhabited by Europeans. It is not exclusively European in its origin, because it would appear that diphtheria was known amongst the ancient Egyptians. Limiting the history of the disease, however, to the last five or six centuries, I cannot make out that it has been met with anywhere outside European influence. If we contract the period of observation still further, and examine into the diphtheria of this nineteenth century, I think it will be difficult to find instances of its endemic occurrence among other than European races. And moreover when it has occurred among Europeans, on foreign soil, amongst an alien people, it has been confined almost entirely to the Europeans. If it has extended to the natives, it must have been in a very limited degree. It can have spread to those only in close relation with the Europeans. Indeed I have not met with any accounts, showing that the disease has fastened on the indigens of any country in which it has been engendered by Europeans. It thus stands out in strong relief to other specific poison diseases; and I think it may be considered, therefore, an essentially European disease. (4) If the foregoing observation be well-founded, it should follow that the cause of diphtheria depends on some special peculiarity, or peculiarities, connected with the modes of life of Europeans. (5) This point fixed, we may readily eliminate food, clothing, water, crowding, want, uncleanliness, and general malarias. None of these things can be admitted amongst the elements of differentiation between Europeans and the rest of the world in the matter of diphtheria. They have in fact been long discarded. (6) When the disease has occurred a long way from Europe—in Australia and India for instance—it has occurred *de novo*. This is indisputable. (7) Almost all observers have concurred upon the point that decomposing organic matter is in some way concerned in the causation; while some have considered sewage and night-soil to be directly implicated. In fine the views of all writers either gravitate to, or are compatible with the assumption of, fœcal matter as the cause of diphtheria. (8) The

disease has almost entirely disappeared from the city of Melbourne, and those of its suburbs in which a complete excrement-removal system obtains, during the last three or four years; whilst it still continues to break out with terrible results in the country districts, and in places where locally-generated indigenous malaria cannot be supposed. (9) Human influence must be assumed as a necessary element in the causation. It is a constant. And within the last century it would seem to have been specially restricted to certain forms of civilisation.

When I come to put all these things together, I can see no logical way out of the question, but to conclude that diphtheria is obscurely, but certainly, connected in its origin with human excrement solely. If this deduction be sound, I can conceive of no other efficient way in which the specific poison is conveyed from the source to the individual, but by a vegetable parasite on excrement. And although I am unable to conjecture the precise conditions under which the factors of the poison are set going, I think it is possible to reduce the conditions, so as to bring them within a narrow compass. Thus it is evident that ordinary filth and general surface-pollution of a country, or a locality, cannot be involved; for neither is it a disease of India, China, South America, or other excrement contaminated countries, nor is it an accompaniment of new Gold Fields, or of military camps. [It certainly occurred in the Crimea to a limited extent; and if the idea of excrement as a cause had been then thought of, it would have been a rare chance for tracing out the particular mode of causation.] Wholesale fœcal pollution, therefore, does not offer favourable conditions for the generation of the (assumed) fungus. It seems to me, on a careful retrospect of the question, that, as the disease is associated with dwellings, and not necessarily dirty dwellings, and appears amongst more settled communities, it must be connected with the common privy system. As it is completely disconnected from fœcal accumulations, or depositions, in houses, and on the surface of yards and enclosures—for cleanly and well-ordered isolated households have been invaded, where the supposition of the intervention of contagion may be excluded—inference points plainly to the collection of excrement in the privy. I suspect that the (hypothetical) mildew of diphtheria grows, either on detached portions of the excreta that have lodged on projections, and have not mixed with the mass, or on the fœcal matter which has oozed out into the surrounding soil—most probably the latter. I cannot get any nearer to the solution of the problem; and so I must leave it. I may observe that the view accords with the fact that children are so much more prone to the disease than adults. For independently of the greater effect no doubt produced on the organism of children than of adults, by a given amount of poison germs, it must be apparent that children are exposed to the noxious influences of privies to a far larger extent than their parents. Whereas adults place themselves within the sphere of the distribution of the poison germs once a day and for a short period, children are constantly playing about back yards and are frequently in the privy itself and for a long while. They are also nearer to the ground, which may make a sensible difference in the quantities and qualities of poison germs inhaled. It is possible, also, that if the recorded case of the two families resident in the one house, in which all the members of one family were infected and not one in the other family was attacked, were investigated, it might transpire that though the two families lived under the same roof, they had separate privy accommodation. Either this, or some other circumstance, prevented the two families from inhaling the same proportion of germs. For though something must be allowed for comparative receptivity, I cannot accede to the proposition that the immunity, in such a case, was entirely due to the individuals themselves. It seems more reasonable to assume a difference in the actual amount of poison.

The proposition that diphtheria is a preventible, and an easily preventible, disease, does not altogether depend upon the soundness, or unsoundness, of the first proposition submitted. That proposition may not stand and yet this may. Whatever the cause of diphtheria, it is clearly a very circumscribed local cause; and the discovery of the cause will, therefore, assuredly lead to its easy removal. If the cause be the one I have hazarded, the prevention of diphtheria will be as simple a matter as that of the prevention of dysentery. A complete excrement-removal system and deinfection of the excreta, on the Mosaic principle, must extinguish the disease. I observe by the evidence given before the Royal Commission on Diphtheria, that the families of the Jews of Melbourne have suffered equally with those of the Christians. This would seem to point to the inference, that the excreta-disposal system of that people, in this city, is either the same as that of the other citizens, or that their children have been exposed to infection from the faulty privy, or other, arrangements of contiguous houses. I believe, as I have said before, that when the Jews, as a people, have obeyed the law of Moses in the spirit, and have not been in immediate contact with other nations, they will be found to have remained unscathed, whilst the other races have been scourged with zymotic pestilences.

It was my intention to have extracted some of the evidence given to the Royal Commission, with a view to illustrating the view I have taken as to causation. I find, however, that I cannot do this, effectively, at the fag end of the book. I must content myself with saying that the testimony of every witness examined by the Commission, is quite reconcileable with the hypothesis; whilst that of very many of them is directly in favour of it. One and all recognise decomposing organic matter as being implicated; and the majority consider fœcal matter to be the principal factor of the specific poison of diphtheria.

It only remains to add that I have debated within myself, whether it would be prudent to risk all the foregoing speculative thoughts as to causation. I have had the ever recurring dread lest, by aiming at too much, I should impair whatever force there may be in the sounder and more solid portions of the argument. I cannot but see that the harping continually on the one string, may beget a feeling of distrust in the reader. It may have too much the air of an attempt to reduce everything to one shape—to open, or else to force, every door with the one key. It may seem, in fact, very like riding a hobby to death; or may even lead to the suspicion of a mild form of monomania. All this has occurred to me, and has caused me to doubt, at times, whether it would not have been wiser to have confined myself to the carrying out of my original design; which was simply to propound the theory as to the cause, and to detail the views as to the prevention, of dysentery. I have now and then thought that there would have been a better prospect of establishing that matter, if I had devoted myself to it exclusively; whereas by involving myself in a series of hypotheses connected with the origin of so many diseases, I may have, possibly, prevented the early consideration, and final acceptance, of a theory, which appears to me to be based on a sure foundation.

On the other hand, when I have reflected on the inseparable connection between dysentery and other camp diseases, it has occurred to me that it would be impossible, or would argue sheer stupidity, to treat of the causation of the flux, without adverting to the causation of the fevers. It further seemed almost incumbent on me to offer some reasonable explanation of the mode of communication between the source of the disease and the persons infected. This led to the mildew hypothesis; and in working out some of the rather intricate problems connected with these hypothetical mildews, I soon became alive to the fact, that I could not possibly be right in assuming these fungi as the efficient causes of any one of these diseases, unless they would also apply to all the other diseases having the same, or a similar, origin. To have left them out of consideration, also, would have been to exclude several things that seemed to be valuable in the way of illustration. It appeared necessary, in fine, to take a more comprehensive view of this whole class of affections; and thus I have thought it advisable to run the gauntlet, and to take the chance of being set down as a visionary, rather than to adhere to the bare theory of the cause of dysentery. And whether the mildew hypotheses stand or fall, the true exposition of the causation of any one of the diseases specially treated of, must involve the immediate discovery of the causation of the others. They are so closely bound together by the tie of a kindred origin, that to know one is sure to lead to knowing them all.

Though I have not dealt with the causation of the exanthemata, influenza, and other specific diseases, I by no means exclude them from the views as to mildews. I have not attempted to work out the problems of their causation; but from the superficial glance I have been able to throw over the subjects, it strikes me that one or two of them might readily be connected as to origin, with mildews on organic matter, and probably excrementitious matter, either of man or animals. Scarlet Fever, especially, appears to be connected with human excrement. Phytologists and Comparative Physiologists and Pathologists must take a stride, however, before anything tangible can come of further speculations as to fungi.

In dealing with the hypotheses of causation, it will have been observed, perhaps, that I have avoided all collateral questions as to the spontaneous production, or the genesis, of germs. The subject has cropped out here and there incidentally, but I have given it a wide berth; not that I am insensible to the interest and importance attaching to it, but I could not afford to be turned from my more immediate purpose. It is a large subject of itself, and I would not complicate the other questions by alluding to it. The view I have taken of genesis, in so far as it has related to these questions of causation, is a simple and practical one. Either the development of the germs which cause endemic disease is a spontaneous generation, or the germs of the germs are omnipresent. One of these two things must be assumed for the occurrence of an endemic outbreak of dysentery, on board ship for instance. Whichever view may be taken is immaterial, so far as the explanation of the development of the dysentery mildew is concerned. There is the fact of the dysentery. If the dysentery be shown to be caused by a mildew on excrement, under certain physical conditions, it is all that is essential to establish the correctness of the hypothesis of mildew causation. The cause of the mildew itself is a thing apart. Therefore I have left it to those who have made such questions their special study.

To conclude. It is some consolation to reflect that whether or not the discussion of the large subjects in this little volume lead to any practical good, at all events it is not likely to end in any serious harm.

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